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Vol. 105
No. 2717

Pages 79-108

January 24, 1947

Science

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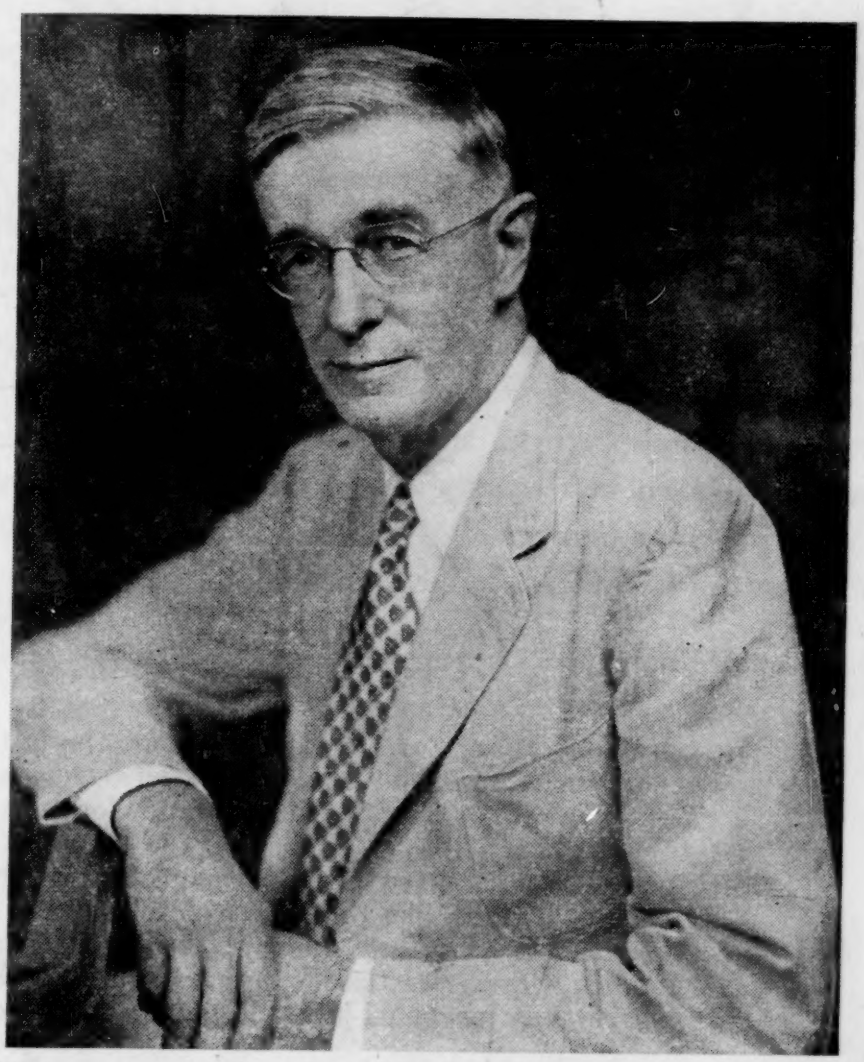

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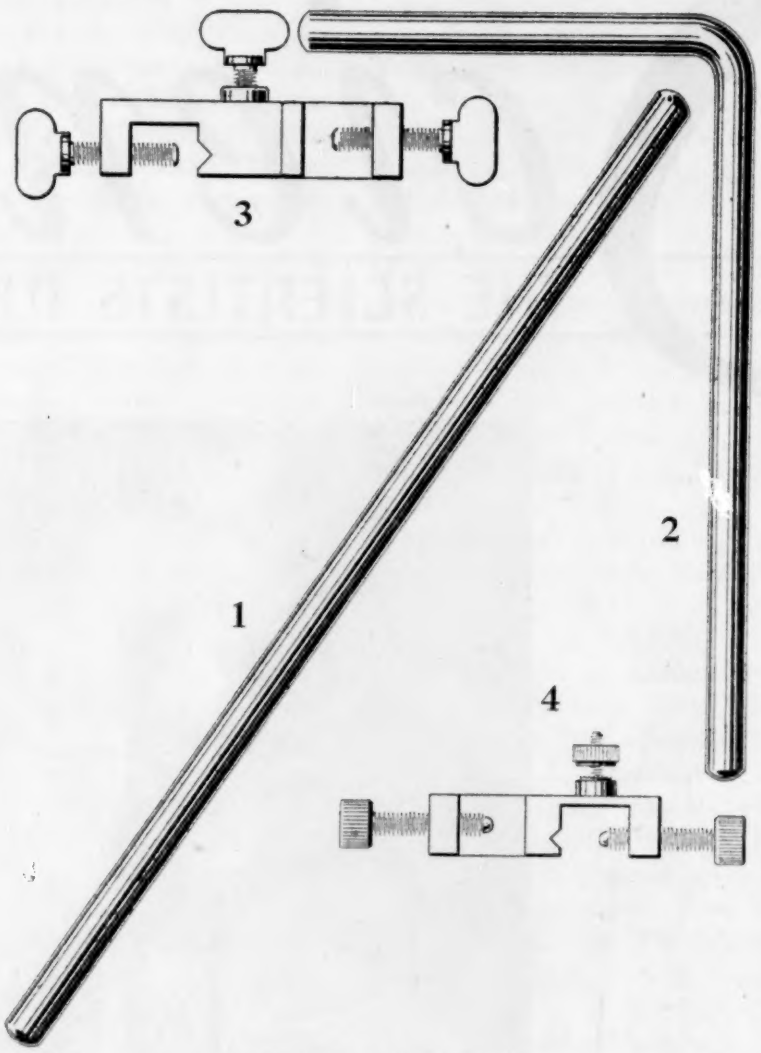
The Machine, the Worker, and the Engineer

Robert K. Merton

For PHYSIOLOGICAL *and other* LABORATORIES



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Science

Vol. 105 No. 2717 Friday, January 24, 1947

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(Cover photo by Harris Q. Ewing)

SCIENCE: A Weekly Journal, since 1900 the official organ of the American Association for the Advancement of Science. Published by the American Association for the Advancement of Science every Friday at Baltimore, Maryland.

Editorial Advisers: W. L. Hart, Karl L. Darrow, Farrington Daniels, Otto Struve, H. R. Aldrich, George A. Baitsell, William Crocker, F. H. H. Roberts, Jr., Robert H. Seashore, Edward P Hutchinson, Chauncey D. Leake, J. C. Hunsaker, W. J. Nungester, W. A. Albrecht, W. C. Olson, W. F. G. Swann.

Communications relative to articles offered for publication should be addressed to Editor, 1515 Massachusetts Avenue, N.W., Washington 5, D. C.

Communications relative to advertising and subscriptions should be addressed to THEO. J. CHRISTENSEN, Mt. Royal and Guilford Avenues, Baltimore, and 1515 Massachusetts Avenue, N.W., Washington 5, D. C.

Communications relative to membership in the Association and to all matters of business of the Association should be addressed to the Administrative Secretary, AAAS, 1515 Massachusetts Avenue, N.W., Washington 5, D. C.

Annual subscription, $7.50 Single copies, 25 cents

Entered as second-class matter July 18, 1923 at the Post Office at Lancaster, Pa., under the Act of March 3, 1879.

(Application for re-entry to the second class of mail matter under the act of March 3, 1879, pending at Baltimore, Md.)

The Machine, the Worker, and the Engineer

Robert K. Merton, *Department of Sociology, Columbia University, New York City*

TO SUSPECT THE FULL MEASURE of one's ignorance is the first step toward supplanting this ignorance with knowledge. What is known about the effects of changes in the methods of production upon the problems, behavior, and perspectives of the worker is little indeed; what needs to be known is very great. A short paper dealing with this large subject can at best roughly map out the contours of our ignorance. It is possible only to allude to the order of research findings now at hand, the conditions needed for suitable extension of these findings, and the social organization of further research required to achieve these results.

So widespread and deep-rooted is the belief that technological advance is a self-evident good that men have largely failed to look into the *conditions of society* under which this is indeed the case. If technology is good, it is so because of its human implications, because large numbers of diversely placed men have occasion to regard it as such in the light of their experience. And whether this occurs depends not so much upon the intrinsic character of an advancing technology, which makes for increased capacity to produce an abundance of goods, as upon the structure of society which determines which groups and individuals gain from this increased bounty and which suffer the social dislocations and human costs entailed by the new technology. Many, in our own society, find the pluralistic social effects of the progressive introduction of labor-saving technology to be far from advantageous. Limited as they are, the data on technological unemployment, displacement of labor, obsolescence of skills, discontinuities in employment, and decreases in jobs per unit of product all indicate that workers bear the brunt of failures to plan the orderly introduction of advances in the processes of production.

Research on these matters is not, of course, a panacea for the social dislocations ascribable to the present methods of introducing technological advances; but research can indicate the pertinent facts of the case—that is to say, it can set out the grounds for decisions by those directly affected by the multiform effects of technological change. Social research in this field has been impressively limited, and it will be of some interest to consider why this is the case.

We shall first review the order of findings which have resulted from social research in this general field; then consider some factors affecting the social role of engineers—especially those immediately concerned with the design and construction of the equipments of production—and the social repercussions of their creative work; and finally, suggest some of the more evident problems and potentialities of further research on the social consequences of labor-saving technology.

From an address delivered before the Conference on Engineering and Human Affairs, Princeton University Bicentennial, October 3, 1946.

Social Consequences of Changes in Technology

Research has detected some of the social repercussions of technological change, a few of which will be mentioned here. These range from the most direct effects upon the nature of work life—the social anatomy of the job—to those which bear upon the institutional and structural patterns of the larger society.

Social Anatomy of the Job

It has become plain that new productive processes and equipment inevitably affect *the network of social relations* among workers engaged in production. For men at work in the factory, the mine, and, for that matter, on the farm, changes in methods of production elicit changes in work routines which modify the immediate social environment of the worker. Modifications of the size and composition of the work team; the range, character, and frequency of contact with associates and supervisors; the status of the worker in the organization; the degree of physical mobility available to him — any and all of these may be collateral effects of the technological change. Although these shifts in the local structure of social relations diversely affect the level of employee satisfaction with the job, they are often unanticipated and unregarded.

The conditions under which such a change is introduced have also been found to determine its impact upon workers. Responding to depressed economic conditions by the introduction of labor-saving technology, management may widen and deepen local pools of unemployment at the very time when workers have few alternatives for employment. Management may thus nourish *the job insecurities and anxieties of workers.* Circumstances such as these understandably lead organized labor to seek a greater part in shaping plans for the introduction of new equipment and processes.

In this connection the tempo of technological change is of critical, though not exclusive, importance. Workers, like executives, seek some measure of control over their

day-by-day lives. Changes imposed upon them without their prior knowledge and consent are regarded as a threat to their well-being in much the same fashion as they are by the businessmen subjected to the vicissitudes of the market or to what they consider "unpredictable decisions" by "those bureaucrats in Washington." Not uncommonly, the worker's stake in the decision has been conscientiously and unrealistically neglected by a management installing labor-saving technology in an effort to maintain or to improve the competitive situation of the firm. It has been observed that an environment of uncertainty, fear, and hostility may be skillfully created by quickening the pace of unpresaged changes in technology.

Through the *enforced obsolescence of skills*, labor-saving technology produces acute psychological and social problems for the worker. The difficulty does not lie exclusively in the need for learning new routines of work. The need for discarding acquired skills and, often, the accompanying demotion of status destroys the positive self-image of the worker, stemming from the confident use of those skills. Although this human cost of new methods of production can on occasion be reduced for individual workers through the planned reallocation of jobs, this does not preclude basic changes in the occupational structure of industry at large.

With technological advance, the growing subdivision of work tasks creates numberless new occupations for which, as Roethlisberger has observed, "there exist no occupational names that have any social significance outside of the particular industry, factory or even department in many cases." The splintering of work tasks involves *loss of public identity of the job.* Who but a chosen few, for example, can distinguish a fin sticker in an automobile plant from other radiator-core assemblers? Or, to take a more homely instance, what distinguishes the pride in work of a doughnut sugarer from that of a doughnut pumper, who successfully injects jelly into fried doughnuts with a jelly pump? To the outside world, these esoteric specializations are all of a piece and, consequently, for the outside world there must be other marks of status and significant work activity that count. The alienation of workers from their job and the importance of wages as the chief symbol of social status are both furthered by the absence of social meaning attributable to the task.

Increased specialization of production leads inescapably to a greater need for predictability of work behavior and, therefore, for *increased discipline in the workplace.* The meshing of numerous limited tasks requires that the margin of variation of individual behavior be reduced to a minimum. This trend, first made conspicuous in the beginnings of the factory system by the rebellions of workers against the then unfamiliar discipline of factory life, has become steadily more marked. In practice, this comes to mean an increasing quantum of discipline which, under specified conditions, becomes coercive for the worker.

Institutional and Structural Effects

The political and social, as well as the economic, by-products of an advancing technology variously affect the structure of society at large. This wider context suggests that workers' attitudes toward the new technology are not determined by it *per se*, but by the collateral uses to which it can be and, at times, has been put as an *instrument of social power.* Technology has been employed not only for the production of goods but also for the management of workmen. It has, in fact, been repeatedly defined as a weapon for subduing the worker by promising to displace him unless he accepts proffered terms of employment.

In the present day, this tactical use of technology in the 'price war' between management and labor need not be phrased as a threat but merely as an observation on the self-contained workings of the market. In an address before this Conference, for example, it has been stated that "among the compelling pressures that now stimulate management to increased mechanization and technological improvement in the processes of production are fantastic increases in money wages, the abandonment or reduced effectiveness of incentive wages, the intransigence of many labor groups, and an abundant supply of cheap money. Process engineers, tool designers, tool makers are now and will be in demand as never before. Invention and innovation will be at a premium without precedent."

A hundred years ago, these political implications of technology (and of the role assigned to engineers) were somewhat more plainly drawn by enterprisers and their representatives. Andrew Ure, for example, could then describe the self-acting mule as a "creation destined to restore order among the industrious classes. . . . The invention confirms the great doctrine already propounded that when capital enlists science into her service the refractory hand of labor will always be taught docility."

It would be instructive to learn if the avowed or tacit use of technology as a weapon in industrial conflict does in fact break the "intransigence" of workers or instruct them in the virtue of "docility." It is possible, of course, that the planned efficiency of a new machine or process is at times unrealized when its collateral function is that of keeping workmen in their place. Quite conceivably it may be found that the exercise of naked power no more produces a stable structure of social relations in industry than in other spheres of human behavior.

Advances in methods of production, as Elliott Dunlap Smith and Robert S. Lynd, among others, have observed, may enlarge the social cleavage between workmen and operating executives. It may produce a sharper *social stratification of industry.* As the complexities of the new technology make technical education a prerequisite for

the operating executive, the prospect of workers rising through the ranks becomes progressively dimmed. To the extent that opportunities for higher education are socially stratified, moreover, managers come increasingly to be drawn from social strata remote from those of workers. Also, since technically trained personnel enter industry at a relatively high level, they have little occasion to share the job experience of workers at an early stage of their careers and tend, accordingly, to have an abstract *knowledge about* rather than a concrete *acquaintance with* the perspective of workers. Finally, with the increasing rationalization of managerial procedures, the relations between operating executives and workmen become increasingly formalized and depersonalized.

These several patterns—progressive closure of opportunities for promotion, the polarization of social origins of workers and executives, the insulation of managerial personnel from workers' outlooks through changes in their typical career patterns and depersonalization of contact—may in composite contribute to a secular trend toward growing tensions between the men who manage and the men whom they manage.

The impact of technology upon the social organization is not, of course, confined to these subsurface trends in class structure. The interdependence of the industrial structure, tightened by applications of science to industry, infects the decisions of large industrial firms with the public interest. In consequence, government comes increasingly to regulate and to supervise these decisions, at least at the margins where they plainly affect the larger community. This trend toward "big government" forces upon popular attention what analytical observers have long recognized: the spheres of economic and political behavior, far from having only tangential relations, overlap considerably. Labor and management deal not only directly with each other through collective bargaining and administrative decision but also indirectly by exerting pressure upon government. Following in the footsteps of entrepreneur and management, labor enters politics.

The growing requirements of work discipline, deriving from technological integration, go far toward explaining the strategic role of the "big union" in our society. "Big industry" has been finding it more expedient or efficient to deal with unions than with large masses of unorganized workers. For industry has come to learn that discipline is often more effectively achieved with the aid of unions of the workers' own choosing than through exclusive resort to the managerial and supervisory apparatus. Moreover, a condition of technological tenuousness in which the stoppage of any one sector of production threatens to paralyze the entire industry modifies the constellation of power relations. All this confers heightened power and responsibility upon labor.

This cursory review of certain consequences of changes in the techniques of production helps sharpen the moral dilemma involved in the choice of problems for social research in this field. Research focused solely on the impact of new technology upon the *immediate work situation* in a plant leads primarily, if not exclusively, to findings which can be readily adapted for making the technological change more acceptable to the individual worker, though it may, in fact, have adverse consequences for him. The scientific problem may be inadvertently construed as one of discovering methods for accommodating the worker to the change, almost irrespective of the mosaic of consequences which it entails for him and his associates. Capital may also enlist *social* science to teach the worker the value of docility. On the other hand, only through this close study of immediate effects upon work life is one likely to discover methods of introducing changes in methods of production which may appreciably mitigate consequences unfavorable to the worker.

Attention solely to the effects upon *the larger social structure* has its limits as well. Research oriented wholly toward secular trends—for example, the pattern of increases in productivity outrunning increases in total employment—diverts attention from ways and means of minimizing the present impact of technological change upon the worker. This type of research, however, does locate the central sociological problem: discerning the features of our social organization which militate against technological progress resulting in "greater security of livelihood and more satisfactory living standards."

Implications for "The" Engineer

New applications of science to production by the engineer, then, do not merely affect the methods of production. They are inescapably social decisions affecting the routines and satisfactions of men at work on the machine and, in their larger reaches, shaping the very organization of the economy and society.

The central role of engineers as the General Staff of our productive system only underscores the great importance of their social and political orientations: the social strata with which they identify themselves; the texture of group loyalties woven by their economic position and their occupational careers; the groups to whom they look for direction; the types of social effects of their work which they take into account—in short, only by exploring the entire range of their allegiances, perspectives, and concerns can engineers achieve that self-clarification of their social role which makes for fully responsible participation in society.

But to say that this poses sociological problems for "the" engineer is to make a reference so inclusive and vague as to mean little at all. The large and multifarious family of men called engineers have a far-flung kinship, but they also have much that marks subgroups off, each from the others. There are military, civil, mechanical, chemical, electrical, and metallurgical engineers, and so on down through the hundreds of titles found among the members of national engineering societies. But whatever

their specialty, so long as they are concerned with the design, construction, or operation of the equipments and processes of production, they are confronted with social and political implications of their position in our society.

A nascent trend toward full recognition of these implications is curbed by several obstacles, chief among which, it would seem, are (1) the marked specialization and division of scientific labor, (2) the applications of professional codes governing the social outlook of engineers, and (3) the incorporation of engineers into industrial bureaucracies.

Specialization

The intensified division of labor has become a splendid device for escaping social responsibilities. As professions subdivide, each group of specialists finds it increasingly possible to "pass the buck" for the social consequences of their work, on the assumption, it would seem, that in this complex transfer of responsibility there will be no hindmost for the devil to take. When appalled by resulting social dislocations, each specialist, secure in the knowledge that he has performed his task to the best of his ability, can readily disclaim responsibility for them. And, of course, no one group of specialists, the engineer any more than the others, alone initiates these consequences. Rather, within our economic and social structure each technological contribution meshes into a cumulative pattern of effects, some of which none has desired and all have brought about.

The Professional Ethic

Deriving in part from the specialization of functions, engineers, not unlike scientists, come to be indoctrinated with an ethical sense of limited responsibilities. The scientist, busy on his distinctive task of carving out new knowledge from the realm of ignorance, has long disclaimed responsibility for attending to the ways in which this knowledge was applied. (History creates its own symbols. It required an atomic bomb to shake many scientists loose from this tenaciously held doctrine.)

So, in many quarters, it has been held absurd that the engineer should be thought accountable for the social and psychological effects of technology, since it is perfectly clear that these do not come within his special province. After all, it is the engineer's "job"—note how effectively this defines the limits of one's role and, thereby, one's social responsibility—to improve processes of production, and it is "not his concern" to consider their ramified social effects. The occupational code focuses the attention of engineers upon the first links in the chain of consequences of technological innovation and diverts their attention, both as specialists and as citizens, from succeeding links in the chain as, for example, the consequences for wage levels and employment opportunities. "But we have to include consequences impartially"—this is John Dewey putting the issue in more general form. "It is willful folly to fasten upon some single end or consequence which is liked, and permit the view of that to blot from perception all other undesired and undesirable consequences."

Bureaucratic Status

The employment of large numbers of engineers and technologists in industrial bureaucracies further shapes their social perspectives. Knit into a bureaucratic apparatus, many engineers take their place as experts in a subaltern role with fixed spheres of competence and authority and with a severely delimited orientation toward the larger social system. In this status, they are rewarded for viewing themselves as technical auxiliaries. As such, it is not their function to consider the human and social consequences of introducing their efficient equipments and processes or to decide when and how they are to be introduced. These are matters for administrative and managerial concern.

The grounds for assigning these concerns to administrators in business and industrial organizations have seldom been stated as lucidly and instructively as in the following passage by Roethlisberger: "... physicists, chemists, mechanical, civil, chemical engineers have a useful way of thinking about and a simple method of dealing with their own class of phenomena. Within this area their judgments are likely to be sound. Outside it their judgments are more questionable. Some of them recognize quite clearly this limitation. They do not want to be concerned with the human factor; they want to design the best tool, the best machine to accomplish certain technical purposes. Whether or not the introduction of this tool or machine will involve the layoff of certain employees, quite rightly, is not their concern as engineers... These men are invaluable to the administrator in any industrial organization."

Max Weber and Thorstein Veblen, among others, have pointed to the danger that this occupational perspective, involving the rationalized abdication of social responsibility in favor of the administrator, may be transferred by engineers beyond the immediate economic enterprise. From this transference of outlook and the resulting trained incapacity for dealing with human affairs there develops a passive and dependent role for engineers and technologists in the realm of political organization, economic institutions, and social policy. The citizen-self threatens to become submerged in the occupational-self.

As technical specialists thus attend to "their own" limited tasks, the over-all impact of technology upon the social structure becomes nobody's business through default.

The Needs of Social Research

Engineers may well continue to abjure any direct concern with the social effects of an advancing technology as

long as the effects cannot be anticipated and taken into account. To the extent that social scientists have failed to address themselves to this problem, there is no informed basis for the most socially oriented of technologists to act with due social responsibility. Only when those equipped with the skills of social research make available an adequate body of scientific knowledge can those working with the skills of engineering extend their sights from the individual business enterprise to the larger social system.

Just as men for centuries neglected the problems of soil erosion, in part because they were unaware that erosion constituted a significant problem, so they are still neglecting the social erosion ascribable to present methods of introducing rapid technological changes. There is a severely limited market for research in this field. It seems safe to suppose that fewer man-hours of research activity are devoted to the intensive investigation of these problems central to our technological age than, say, to the design of alluring packages for perfumes and other such basic commodities or to the planning of competitive advertisements for the tobacco manufacturers of the Nation.

The inauguration of a vast program of social inquiry proportioned to the scale of the problem need not wait upon new research procedures. Methods of social research have been advancing steadily and will undoubtedly become developed further through disciplined experience. The effective development of this program does wait, however, upon decisions concerning the organization of the research teams, sponsorship of the research, and the directions of inquiry.

Organization of the Research Team

Disparate and uncoordinated inquiries by diversely skilled groups have not proved adequate. The problems in this area call for the complementary skills and knowledge of engineers, economists, psychologists, and sociologists. Once this focus of joint inquiry is recognized, systematic efforts to institute a program of collaborative investigation could be begun by representatives of the several professional societies. Common universes of discourse would probably be lacking at the outset, but, as the experience of the TVA suggests, patterns of collaboration between engineers and social scientists can be evolved. The walls insulating the several disciplines raised up by the division of scientific labor can be surmounted if they are recognized for the temporary expedients that they are.

Sponsorship of the Research

Of the limited body of social research in industry, the greater part has been oriented toward the needs of management. The problems selected as the focus of the inquiry—high labor turnover and restricted output, for example—have been largely those defined by management, sponsorship has been typically by management, the limits and character of experimental changes in the work situation have been passed upon by management, and periodic reports have been made primarily to management. No matter how good or seemingly self-evident the reason, it should be noted that this *is* the typical perspective of social research in industry and, as such, limits the effective prosecution of the research.

These remarks do not, of course, impugn the validity and usefulness of research oriented toward the needs of management. From the fact that this research continues to be sponsored by management, we can conclude only that it has been found eminently useful and valid, within the limits of the definition of problems. But an intelligence staff for one stratum of the business and industrial population may in due course find itself focusing on problems which are not the chief problems confronting other sectors of that population. It may happen, for example, that devising methods of reducing workers' anxieties through sympathetic and prolonged interviews or through appropriate behavior by supervisors is not among those researches which workers regard as central to their interests. They may be more concerned with having research men uncover the varied consequences, for themselves and for others, of alternative plans governing the introduction of technological changes.

This reminds us that social research itself takes place within a social setting. The social scientist who fails to recognize that his techniques of participant-observation, interviewing, sociogramming, and the like represent an innovation for workers and supervisors greater, perhaps, than technological changes in the plant would indeed be a dubious believer in his own findings. Resistance to this innovation can be anticipated, if only because it is remote from run-of-the-mill experience of most people. Those who have engaged in social research among workers and administrative personnel need not be told of the mingled suspicion, distrust, uneasy amusement, and, often, open hostility with which they were initially met. Unfamiliarity with this type of inquiry, coupled with its apparent inquisitiveness into tension areas and private affairs, makes for some measure of resistance.

If the research is subsidized by management and if the problems dealt with are relevant primarily to management, the resistance of workers will be all the greater. It is small wonder that in some quarters of organized labor the preliminary efforts at social research in industry are regarded with a measure of suspicion and distrust comparable to that which attended the introduction of scientific management studies in the 1920's. For if workers have occasion to identify the research program as a newfangled academic device for countering labor organizations or for scientifically substituting symbolic for material rewards, it will create rather than locate problems.

Social research in industry, therefore, must be conducted under the joint auspices of management and

labor, irrespective of the source of funds for the research. The cooperation of large numbers of workers will not be achieved unless they know that they will be beneficiaries of an application of scientific method to a field where rule-of-thumb has largely prevailed.

The Directions of Research

The initial task of these research teams would be to search out the specific problems which demand attention. The very fact that they undertake the research would indicate that they are not possessed by the opaque faith that forward strides in technology, howsoever applied, must lead to the common good. They would be expected to think dangerous thoughts. They would not hold cultural and institutional axioms to be beyond inquiry. The focus of their attention would be the institutional arrangements adequate to incorporate the full potentialities for production of an unevenly but continuously advancing technology with a socialization of gains and losses contained in these advances.

During the last decade there has occurred a reaction among social researchers against the earlier tendency to focus on the economic consequences of advances in technology. The center of research attention was shifted to workers' sentiments and social relations on the job. This new emphasis, however, has the defects of its qualities. It is not only the sentiments of workers which are affected by technological change; it is not only their social ties and their status—it is also their incomes, their job chances, and their economic interests. If the new research on human relations in industry is to have maximum pertinence, it must be meshed with the continuing research on the economic implications of labor-saving technology.

Nor can the research be effectively confined to studies of "the worker." To single out the worker as though he represented a self-contained sector of the industrial population is to do violence to the structure of social relations which actually obtains in industry. Presumably, it is not only the worker who is subject to preoccupations, obsessive reveries, defects and distortions of attitude, and irrational dislikes of co-workers or supervisors. It might even turn out that the behavior and decisions of management are appreciably affected by similar psychological patterns and that these, as well as a clear-cut sense of economic interests, go far toward determining decisions on the introduction of labor-saving technology.

In the absence of research jointly sponsored by labor and management and aimed at commonly agreed-upon problems of the role of technology in our society, the alternative is to pursue the present pattern of piecemeal research, directed toward those special problems which it is in the interest of special groups to have examined. It is possible, of course, that this alternative will seem preferable to some. It is altogether possible that the several interested groups will find no basis for agreement on the sponsorship and direction of social research in this field. But then, this too would serve its backhanded purpose. Should research by technologists and social scientists under the joint auspices of management and labor be rejected on these grounds, it would be a significant diagnostic sign of the state which industrial relations have reached.

The Federal Government and the Shortage of Scientific Personnel

Charles V. Kidd
President's Scientific Research Board Staff, Washington, D. C.

THE RESEARCH AND DEVELOPMENT program of the Federal Government is, for a number of reasons, as large as, and in many fields larger than, during the war. Current expenditures on research and development, which are at about 20 times the 1940 level, will approximate $1,500,000,000 during the year from July 1946 to July 1947, excluding expenditures on the Manhattan Project. This means not only that the Federal Government is conducting or financing a very large share of the total research and development now undertaken in the country but that Federal policies with respect to research affect, directly or indirectly, every industrial laboratory, university, and individual scientist.

The distribution of these expenditures, as well as their total volume, is significant. Since about 90 per cent of the total research and development expenditures of the Federal Government are accounted for by the War and Navy Departments, development bulks heavy in the Federal program. Construction of prototypes and similar work is very expensive when compared with expenditures on fundamental research, and the armed forces have a direct and primary interest in those applications of science directly related to improvement of the ability of the services to fight an effective war. This concentration of Federal expenditures on developmental work raises significant questions relating to the

The substance of this paper was presented before Section K, AAAS, at the Boston meeting, December 28, 1946.

rate at which the fundamental knowledge on which development is based can be pushed ahead.

The distribution of the Federal research and development effort among objectives, particularly those that can be classified as military and nonmilitary, is also a matter of national interest. Expenditures on research related to human welfare are large in absolute terms, but small in relation to all Federal research and development expenditures. The Public Health Service has a budget of $11,000,000; the Department of Agriculture, $37,000,000; and the Bureau of Standards, $9,000,000. To these, however, must be added very substantial sums that will be spent by the War and Navy Departments on research in prosthetic devices, medical research, and other work contributing to improved civilian goods and processes. Whether the balance between military and nonmilitary research is a sound one—giving due weight to the urgency of developing more effective instruments of war—is a question that deserves most careful scrutiny.

Finally, the administrative arrangements for the conduct or financing of research by the Federal Government ultimately not only affect the efficiency of work in Federal establishments but also bear directly upon research and development conducted by industry and universities.

Of the numerous questions involved in the administration of research by the Federal Government, some of the most perplexing arise out of the practice of contracting research. During the war it was clearly of vital importance to mobilize all of our scientific resources for effective prosecution of the war. The technique of contracting, widely extended as a mobilization measure, proved to be most effective, and Federal contracts for both fundamental research and development are now being granted on a scale unprecedented in peacetime.

There is, however, the problem of deciding how much, if any, research and development should be done with Federal funds in Federal establishments. No over-all set of guiding principles now exists in this important area.

The consequences of lack of policy are serious. Contractors can offer salaries to Federal employees far in excess of those that the Government can pay. In effect, the Federal Government competes against itself for scarce talent under an extensive contracting system and decides, in effect, to deplete government establishments of good men in order to staff contractors.

Extensive contracting also raises problems of budgetary control. Conduct of research and development under contract rather than in government laboratories frees the project of the many controls over expenditure that are commonly considered necessary in conducting the affairs of the Federal Government. On the other hand, Congress decides what the executive branch ought to do by enacting enabling legislation and appropriations. The Bureau of the Budget was established to ensure that appropriations are spent for the purposes intended by Congress. How far, and under what circumstances, contracting agencies and contractors should be in effect exempted from the usual fiscal controls are matters deserving the most careful study.

In Federal contracts for fundamental research with universities, as contrasted with developmental work, the questions of freedom of research and freedom of discussion are always present. Whether, in the long run, a large proportion of the Nation's fundamental research can be financed by the Federal Government—and particularly by the War and Navy Departments—without influencing the direction of research or without unduly circumscribing, in the name of national security, freedom of discussion among scientists remains to be seen. At the moment, the Federal Government is providing much of the money necessary to prevent a severe decline in the volume of badly needed fundamental research. One of the problems for the future is to decide at what level the support ought to continue and to establish uniform policies ensuring that the research will be free.

Executive Order 9791

The President recognized these problems, and on October 17 Executive Order 9791 was issued.

Under this Order, the Assistant to the President, Dr. John R. Steelman, is directed to undertake a thorough study of the Federal research and development program. He must inventory and classify the major fields of research and development conducted or financed by the Government. The types and numbers of personnel now employed and required eventually for execution of the programs must be determined, and the cost of the program analyzed.

To place the Federal program in proper perspective, the Executive Order directs a review, from readily available sources, of the same sorts of information for non-Federal research—primarily work in process and planned by industries, foundations, and universities with their own funds. The objective of this review will be only that of determining the gross volume of funds devoted to broad areas of research, the total number of scientists and technicians now employed, and the total number needed. This information, rather than a catalogue of the nature and objectives of specific projects, will be adequate to set the Federal program in the framework of the total national program.

On the basis of these studies, the Assistant to the President is to prepare a report to the President containing his recommendations for planning, staffing, and administering the Federal research programs to ensure that the scientific personnel and training and research facilities are used most effectively in the national interest.

The Executive Order established the President's Scientific Research Board, composed of the Assistant

to the President as chairman, Cabinet officers whose departments are engaged in research, and the heads of Federal research agencies (*Science*, November 1, p. 418; December 17, p. 616). On January 2 Dr. Steelman announced the appointment of Dr. J. Donald Kingsley, formerly deputy director of the Office of War Mobilization and Reconversion, as executive secretary of the Board and of a committee of alternates composed of: Maj. Gen. H. S. Aurand, chief, Army Division of Research and Development, representing the War Department; W. John Kenney, Assistant Secretary of the Navy; Edward U. Condon, director, National Bureau of Standards, representing the Commerce Department; Carroll L. Wilson, general manager, Atomic Energy Commission; Thomas B. Nolan, assistant director, Geological Survey, representing the Interior Department; W. V. Lambert, administrator, Agricultural Research Administration; H. K. Jett, commissioner, Federal Communications Commission; John P. Ferris, representing the Tennessee Valley Authority; Mary E. Switzer, assistant to the administrator, Federal Security Administration; N. S. Fairbanks, deputy commissioner, Federal Works Agency; John W. Crowley, Jr., acting director of aeronautical research, National Advisory Committee for Aeronautics; Paul A. Scherer, representing the Office of Scientific Research and Development; and John T. Cox, Jr., manager, Synthetic Rubber Division, Reconstruction Finance Corporation.

A small staff in the Executive Office of the President will coordinate the study. Most of the staff members will be borrowed from the agencies conducting research. These men will discuss with each agency the scope and nature of the data on research projects required for the study and will work with the agencies in drawing material together. Other staff members will secure information from industry, foundations, and universities.

After, and in some cases while, information is being gathered, interagency committees centering around fields of research will be set up to examine the work being done in specific fields. This task of analysis will be undertaken only in those areas where fields of research have not been adequately analyzed by existing bodies with a view to coordination. The Joint Research and Development Board, for example, is working on, or has completed analyses of, a number of fields of research undertaken by the War and Navy Departments.

Interagency committees will also be set up to examine problems relating to the administration of research. One group will concern itself with personnel problems of the type to be outlined below.

All of these committees will prepare reports outlining the facts with respect to the area in which they have worked and their recommendations. These will be collated, reviewed, and incorporated in a consolidated draft report which will be submitted to the President's Scientific Research Board. Taking into account the comments of the Board, the Assistant to the President will then prepare a report to the President on the Federal research and development program which will deal with a number of basic problems:

What, for example, is the proper role of the Federal Government in the field of fundamental research?

What are the implications of financing a large volume of the total national program of fundamental research by the War and Navy Departments?

What is the impact of extensive Federal research contracts on the speed with which new scientists are being produced?

What is the division of Federal research effort between preparation for war and improvement of the arts of peace?

Can the total volume of research and development contemplated by the Federal Government, industry, foundations, and the universities be competently supported by the talent available? If not, what policies are indicated for the Federal Government?

What the Federal Government Is Doing to Increase the Supply of Scientists

In the long run, the G.I. Bill of Rights will undoubtedly prove to be a most significant means of increasing our supply of scientists. The educational benefits provided under the bill are financing a large part of the college expenses of more than 1,000,000 veterans, a large proportion of whom would not have attended college without this financial aid.

The support of research in universities by the Federal Government, while drawing many competent men from teaching into research, also supplies the funds for projects upon which advanced students can work under the direction of more highly qualified men. The opportunity to apply knowledge and intelligence to specific projects is a definite stage in the development of competent research workers. The Federal funds now being devoted to work in universities are giving a large number of graduate students a chance to advance more rapidly than would be the case if such funds were not available.

Aside from grants for the support of research projects, the Federal Government is financing a substantial number of expensive research facilities to be used by universities, particularly in the field of nuclear physics. These facilities are of direct assistance in expanding the number of advanced research workers who can be trained.

Aside from the G. I. Bill of Rights educational benefits, the Federal Government has begun to finance advanced research workers by fellowships. A National Institute of Health Fellowships program was established by Congress in 1945 under the Public Health Service Act. These fellowships are awarded to individuals who have had postgraduate work in institutions of recognized standing in fields of science allied to public health. Junior research fellowships paying $2,400 a year are

available to those who have Masters' degrees, and senior research fellowships are available to those holding Doctors' degrees. The grants are for one year and are renewable under some circumstances. The fellows select the fields within which they will work as well as the institutions where they will work. Thus far, about 100 fellowships have been granted.

Finally, the Federal Government is financing the higher education of able men under the new Navy Reserve Officers Training Program, and the War Department is contemplating the establishment of a similar program.

The entrance of the Federal Government into the field of financing the general and particularly the specialized higher education of individuals is a development of the highest significance. Grants for advanced study are now made on a small scale. Funds for general education are confined to a temporary program for veterans and to a permanent program for Navy officers. But these statutes may well mark the first steps in the establishment of a broader program of Federal aid both to educational institutions and to individual students.

Even though, in the long run, the national welfare depends on broadening the educational base, there are dangers in an uncoordinated, piecemeal program of Federal support to individuals. It is questionable, for example, whether the Federal Government should concentrate solely upon support of students who will become scientists, or Army and Navy officers. There are potential dangers in the uncoordinated proliferation of plans that will draw a large proportion of the most highly qualified young men into the physical sciences or the armed forces.

This question, among others relating to basic questions of national educational policy, is now being considered by the President's Commission on Higher Education.

The Federal Government as an Employer of Scientists

There is little prospect that the shortage of scientists will be relieved in the near future by a reduction in the personnel required to staff research and development programs conducted or financed by the Federal Government.

Given a job to do, Government should, as is true of any employer, try to get the work done as well and as cheaply as possible. This means that the Government ought to attract good men and establish the working conditions that will make them most productive.

Among scientists in general, Civil Service employment has been viewed with a skeptical eye. By and large, this skepticism is based on inadequate secondhand information and upon conditions that are quickly being remedied.

The Federal research program does offer a wide and useful field of employment for scientists of junior grade. After advantages and disadvantages are balanced off, scientific work for the Government in the lower and middle brackets is about as attractive as in industry or universities.

There are, however, a number of problems that hamper the recruitment and retention of top scientists. These problems are not universal by any means, but they are of sufficient significance to merit attention.

The personnel procedures of some agencies impose a number of irksome barriers to maintenance of an effective working group of scientists. In general, these barriers arise out of the rigidity with which the personnel mechanism can operate in the Federal Government. In some cases, for example, salary levels for technical and scientific positions are evaluated more in terms of administrative responsibility than in terms of scientific achievement. It is sometimes difficult to reward outstanding work adequately.

Most important, the upper Civil Service limit of $10,000 is a crippling limitation. While many outstanding scientists have remained in the employ of the Federal Government at salaries of $10,000 per year or less, the upper limit both forestalls recruitment of badly needed men and leads to a continuing exodus of capable scientists from Federal employment. It is difficult to reconcile a decision to invest billions of dollars in research with an inflexible ceiling of $10,000 on Federal salaries.

A broader question raised by salary levels in government, industry, and the universities is, of course, the ability and willingness of industry to pay relatively high salaries to men engaged in developmental work at a time when fundamental research is lagging.

Another general problem concerns the opportunity of Federal scientific workers to acquire and enhance their professional status. In many agencies, opportunities to publish research findings are inadequate, and in most agencies, funds for attendance at professional meetings are meager.

Some scientists in the War and Navy Departments face a peculiar and difficult set of circumstances arising out of their working relationships with officers.

Petty, but irritating, discrimination is a problem. When scientists travel, for example, they are allowed $6 per day for per diem; officers are allowed $7 per day plus tips. Housing accommodations have often been provided for officers but not for equally needy civilians. Top-level scientists are sometimes forbidden to sign letters or reports over their own signatures.

A more basic problem has been the supervision of the work of highly competent scientists by officers who do not have a full understanding of the work for which they are responsible.

Security restrictions have at times proved baffling to civilian scientists in a number of respects. Sometimes there seems to be an assumption that a man in uniform

is automatically endowed with infallible judgment and impeccable circumspection, but that a man not in uniform is an outsider who must be carefully watched. On some projects this has led to a formidable and discouraging array of special security checks, special passes, and restrictions on working hours, availability of data, and freedom of conversation which must be borne by civilian scientists but not by officers.

Finally, restrictions on publication or research findings for security reasons have often seemed overly strict to civilian scientists. Officers in a military hierarchy, trained to deal with matters of primary or sole interest to the armed forces, relatively untrained in matters of general scientific interest, subject to military discipline and military justice, and indoctrinated with the importance of keeping military data secret, naturally lean further toward maintenance of secrecy than civilian scientists trained in another philosophy and environment.

Security policies are also, of course, a major problem in connection with War and Navy Department contracts undertaken by scientists in academic institutions. The imposition of security restrictions on the work of scientists who have a cherished tradition of freedom to talk and freedom to write creates an area within which judgments and attitudes almost inevitably differ.

All of the problems of the Federal Government as an employer have been studied intensively during the past year by the Civil Service Commission, by the agencies concerned, and by interagency committees. Many of the difficulties outlined have been remedied, and specific recommendations dealing with the others are being formulated. A number of them, however, are not matters that can be dealt with by law or by a general statement of policy. They are matters of personnel administration which must be worked out by careful and diligent effort attending over a substantial period of time. Furthermore, many of these personnel problems must be worked out within the agencies concerned.

Below are outlined some of the specific actions that have been taken, or are about to be taken, to make the Government a better employer.

The Civil Service Commission will soon appoint a highly placed official whose task will be to deal with all personnel matters affecting scientists and technicians. The establishment of this post will provide a central point for the establishment and implementation of policies designed to enhance the position of scientists employed by the Government.

Recruitment of scientists is being improved. About 750 special boards of Civil Service examiners, on which scientists are represented, will be set up to recruit professional people, including scientists. These boards will recruit scientists, particularly for junior jobs, through contact with schools. They will set up appropriate examinations, establish registers, and certify eligible names for employment.

The salary problem will be attacked by presenting to Congress the urgent need for raising the upper pay limit for both top scientists and administrators from $10,000 to $15,000.

To provide means of professional advancement, to increase the effectiveness of scientists at their job, and, incidentally, to increase the effective supply of scientific talent, an extensive program of in-service training for scientists is in operation and is being extended. In Washington, for example, about 800 scientists from 15 agencies are taking courses in 22 scientific fields (*Science*, November 22, p. 477). The Army Air Corps at Wright Field has developed a similar program, as have Navy installations at New Haven, and Inyokern, California.

The problems of security restrictions and relationships between military and civilian personnel, in areas where these matters have concerned scientists, are being dealt with intelligently as a matter of top policy. There is an increasing tendency, for example, to establish administrative organizations designed to give scientists a greater degree of freedom and more direct access to policy-making officials. The central research and development staffs of both the Army and Navy are organized in a manner designed to enhance the status of civilian scientists. This development is significant not only to Federal scientists but also the utility of the Federal research program.

A great deal of the credit for these steps goes to the Advisory Committee on Scientific Personnel, under the chairmanship of M. H. Trytten. This group has been most effective in presenting both a philosophy and a program of action to administrators. The President's Scientific Research Board will lean heavily upon them for advice and recommendations in this field.

Sound answers to all of the problems faced by the Government as an employer of scientific personnel must be found if it is to do a good job of research and development. But the Government also has a wider responsibility. As a major competitor for one of our most scarce and valuable resources—highly trained scientists—the Federal Government is obliged to consider the effects of its research and development programs on the distribution of the total pool of available man power. Since the national interest is bound up with the progress of research, the Government must ensure that the supply of well-trained men is raised to an adequate level without drawing into scientific and technical fields too large a proportion of the best brains of the country.

The policies to be followed by the Government in dealing with these problems concern not only Federal agencies. Industry, the universities, and the foundations have a vital stake in the establishment of sound decisions by the Federal Government in this area. All scientists will be affected by the actions of the Federal Government as it charts a course to guide postwar research.

NEWS *and Notes*

Vannevar Bush, chairman of the newly formed Joint Research and Development Board, which now coordinates the support of basic research offered by Army and Navy contracts, addressed a general engineering meeting sponsored by 10 engineering societies in Washington on January 14.

He spoke principally of the physical sciences, saying that "another whole evening could be spent on the amazing implications of present advances in the biological sciences; and the social sciences, too, in so far as they contribute to sound bases for human relationships."

Further, he said, "we have attained full public recognition of the basic relationship between science and civilization." Moreover, with basic research, as with applied research, success depends primarily on the man concerned in any research undertaking. He must be able, and he must be free, to put his ability to use as only he can do.

Right here we run into a possible difficulty. Whatever else we can say about research, basic or applied, we know that it costs money, sometimes a lot of money. The research man must be able, and free to put his ability to use. But he cannot be free—indeed, cannot even be fully able—without long and costly training, without materials, equipment, facilities. This means he must have support. The possible difficulty is clear—if support has strings on it that means control of the researcher, the essential freedom goes. If support is not given with full confidence, there is hence a dilemma. If support *is* given with confidence, however, the dilemma need not exist.

The hazards which any formal system of support might involve are few but great. We may cite as principal ones first, the danger of overemphasis on applied science, especially the applied science of immediate benefit to the supporting agency, because of its possible practical returns; and second, injurious dictation by laymen who might misinterpret the power of the purse as giving them skill to tell the researcher what lines to follow. American experience in the support of research, as for instance in both private and state-supported institutions such as universities, has been highly fortunate in that these hazards have in the main been avoided. As we move toward a broader plan of support, some observers worry about the possibility of these dangers as though they were new things and were created by the broader plan. They have, however, always existed, and will always exist as long as the possible dilemma which I have mentioned confronts the research man. By and large, the broader the source from which he draws his support, the less risk of dictation he runs.

In the years before the war, support for research in this country came from four main sources—universities, industry, philanthropic foundations, and the Government, which in several departments, notably Agriculture, Commerce, and Interior, and the Federal Security Agency, maintained research staffs and carried on research programs. Today, these four sources operate as before, but the Government's role has changed importantly. . . . During the war, Federal funds were channeled into research in principal measure through OSRD. At present, the principal flow of funds is through the armed service. In time to come, another means will in all likelihood be provided.

Let us look at the financial side of the matter. The total outlay of the United States in research and development now amounts to more than a billion dollars a year. This is about three times as much as we were spending yearly before the war. Before the war—1940, say—the Government provided about 20 per cent of the total of 345 million. Now, the Government's share is 50 per cent. The principal channels through which this Federal money flows are the Departments of Agriculture, Commerce, Interior, the Navy Department, the War Department, and the Atomic Energy Commission. Note that the armed services, which accounted for about 6 per cent of the much smaller prewar total, are disbursing some 40 per cent of the present total of about $1,077,000,000.

Now, these are important sums, in anybody's bookkeeping. The present pattern of their expenditure places on the armed services a heavy responsibility in both basic and applied research. Much of their responsibility for applied research the services must continue to carry. When proper opportunity offers, they will presumably be glad to relinquish what they can of it. The responsibility they now have for basic research can be lifted. The contracts which the Army and Navy have written with research organizations—not for the devising or improving of weapons of war, but for basic research—have been effective in giving vitally needed Federal support to the country's scientific effort in the interval since the contraction of OSRD work. The support thus rendered has strengthened the universities during a difficult period. It has gone some distance toward filling the deficit in basic research caused by our having had to focus principally during the war on applied research for immediate purposes. It has indirectly contributed somewhat toward redressing our grievous present and future shortage of trained scientific personnel. It is fortunate that the Congress has had the vision to support the services in their performance of this vital interim task, and that the services in turn have had the wisdom to step in where help is needed. *In the long run, of course, it is to be desired that Federal support of basic research be extended rather through a body that balances civil and military needs and is closer to an understanding of fundamental research as a whole than the services, in the nature of the case, can hope to be.* That the services desire such an outcome is plain in the record of their advocacy before the last Congress of legislation to establish a National Research Foundation.

Let me turn now to consideration of the proposal for such a foundation, what it signifies, and what we have by way of experience and knowledge to combine in it. Federal financing of research is by no means a new thing; it dates well back into our history in the actual research operations carried on in several government departments; in the somewhat newer form of support through contracts it is now going on apace. *But the creation of an agency separately and specifically planned to coordinate and administer this newer form of support will be a new thing.* The task is complex enough and important enough to require such an agency. More than that, the temporary system under which we are now operating cannot per-

form some of the important things that must be done. Notable among these is the job of finding able young potential scientists and seeing to it that, wherever they are and whatever their economic status, they have full opportunity for the education and training that will make their abilities available in the expanding programs which we must undertake. . . .

We have a substantial body of practical knowledge on which to draw. Outside of government, the American people have probably had as much experience in the administration of industrial and university research as any other nation. . . .

The Joint Research and Development Board was established last July by joint action of the Secretary of War and the Secretary of the Navy, to have civilian as well as military members, for the purpose of coordinating the research and development programs of the War and Navy Departments. . . . I wish to emphasize that the JRDB, concerned with policies, plans, and allocations of responsibility, does not enter into the affairs which are the proper concerns of G-6 and ONR, respectively. These affairs—the actual operation of research and development programs as well, of course, as their initial formulation—are carried on by the departmental agencies. The Board thus is not an operating body; its principal function can properly be described as a judicial one. It resolves differences which may arise between the War and Navy Departments in the broad field of research and development, and in particular resolves the differences arising when the programs of the two services are merged to form an integrated national program. Since it is expected to reach decisions, it works by majority vote. Obviously, if it is to work in the broadest national interest, it must be fully informed. It has a major supporting activity, therefore, in the assembly and analysis of information and the correlation of research objectives with broader military plans. It thus becomes well qualified to formulate opinions and render advice, and in this respect becomes important in determining relative emphasis, filling gaps, and shaping policies. Except in the allocation of responsibility for programs, its actions take the form of recommendations to the Secretaries.

Membership of the Board consists of Gen. J. L. Devers, Gen. Carl Spaatz, Adm. D. C. Ramsey, Assistant Secretary of the Navy W. John Kenney, and myself as chairman. Its Policy Council, on which the chief planning officers of both departments meet regularly with representatives of the Board, is a vitally important part of the setup. It provides for the correlation of research and development with strategic requirements, in an obviously two-directional way, for new developments may have sweeping implications for strategy. There is, of course, a vast amount of work to be done in achieving the goal of a strong, unified, integrated national program. This work is allocated to committees, of which six have thus far been organized, each covering a major field of technical activity.

The Committee on Atomic Energy is headed by J. B. Conant; it will function in close collaboration with the Atomic Energy Commission. Its membership and that of the military liaison committee and the general advisory committee for the Commission overlap somewhat, a condition that will be helpful.

Hartley W. Rowe is chairman of the Aeronautics Committee.

K. T. Compton is chairman of the Guided Missiles Committee.

The Committee on Electronics has as chairman J. A. Stratton.

The Committee on Geophysical Sciences, concerned with such subjects as meteorology, oceanography, and so on, is under the chairmanship of R. F. Beers.

The field of geographical exploration is coordinated by the sixth committee, headed by Charles Behre.

Others will, of course, be organized as the work of the Board requires. Each of these committees can be considered to follow within its defined area the general pattern of the Board itself. Thus, each committee consists of civilian and military members. Usually, the chairman will be a civilian. The Board has delegated great authority to its committees, when they act on the basis of unanimous decisions. Thus, for example, the Committee on Guided Missiles, acting unanimously, will undoubtedly have the final word in determining allocation of responsibility in the national program in the field of guided missiles. A question on which the committee does not reach unanimous decision will, of course, go to the Board for resolution. Otherwise, only two types of decision of the committee will require review by the Board itself: first, matters involving broad policy or strategy concerning which the Board may consider that the committee's knowledge may have been incomplete; and second, matters transcending the interest of a single committee and hence requiring consideration by more than one agency of the Board.

Since both the Board and its committees are made up of individuals serving part-time, much of the important work is carried on by the full-time staff, which is headed by Lloyd Berkner, executive secretary. He is assisted by Maj. Gen. Anthony C. McAuliffe, the Army Secretary, and Capt. J. H. Thach, the Navy Secretary. . . .

From the point of view of military organization, the Board is a good illustration of a staff unit performing a staff function. It is not an executive body. It is not an administrative body. It is a body where qualified minds can secure information, formulate advice, and resolve differences at the level where they occur. It can therefore be regarded as an important step in the formation of a combined staff—which may well be our next logical progression from the concept of the War Department General Staff which we owe to the genius of Elihu Root, the concept of an organization composed of officers free from routine administrative activities and free from responsibilities of command who can spend their full time in the vital staff functions, of which one is planning.

From the point of view of science—that is, particularly of research—this Board . . . is essentially a peacetime mechanism, seeking to meet conditions requiring a machinery for coordination differing in major respects from that which was adequate during the war. The dollar is no longer flexible, and in a period of limited or diminishing appropriations the task of adequately apportioning emphasis and adjusting programs becomes more difficult. The lessened feeling of urgency also contributes. During war, the penalties for delay were so obvious that it was relatively simple for men to agree on a course of action involving diverse interests. In peacetime it is inevitably more difficult for any group to subordinate its individual interests, and it is correspondingly more difficult to achieve solutions directed toward the broadest national interest.

This situation holds true not only in the area for which the Board has responsibility but also in many others. A result of it is that agencies seeking to reach solutions need the broadest possible base in the scientific and economic life of the country. Government is necessarily as-

suming responsibility for a larger part of the scientific effort of the Nation. That fact means emphatically that there will be increasingly frequent calls on competent citizens for counsel and cooperation, for service on boards and committees, for arduous meetings, for exacting work. If the task is to be properly met, citizens must respond and give their time and energy, making their special professional acumen available to the Government. It is pleasant to meet and pass a resolution and then go home, but it doesn't accomplish much toward working out specific answers to specific problems. . . .

About People

Wesley C. Cox, Colonel, MC, has been appointed chief of the Army Industrial Hygiene Laboratory, Edgewood Arsenal, Maryland, with offices in the Surgeon General's Office, to succeed Maj. Robert H. Duguid, MC, now conducting an industrial medical and hygienic survey of Army Air Forces bases in Europe. Col. Cox returned recently from seven years in Panama, where he had been Canal Department Surgeon since 1943.

Rebeca Milies, professor of psychopedagogy, on leave of absence from the Institutos Normales, Montevideo, Uruguay, has been appointed to an externship at the Wichita Guidance Center, Wichita, Kansas. Prof. Milies, who was awarded maintenance and travel grants by the State Department through the Institute of Internation Education, to study child research and service centers in the United States, previously held an internship at the Institute for Juvenile Research, Chicago, and a fellowship at the University of Iowa Child Welfare Research Station.

Theodore L. Swenson, since 1938 director of the Western Regional Research Laboratory, USDA Bureau of Agricultural and Industrial Chemistry, Albany, California, has been appointed special assistant to Louis B. Howard, chief of the Bureau, the Department of Agriculture has announced. Michael J. Copley, for seven years with the Eastern Regional Research Laboratory, succeeded Dr. Swenson at Albany. Dr. Swenson's new position was created to improve and expand utilization of farm crops and products in processed foods and get results of such food research into commercial use.

Merrill F. Roff has joined the staff ofi the Research Division, Air University School of Aviation Medicine, Randolph Field, Texas, as chief, Department of Psychology. He formerly was director, Aviation Psychology Program, Office of the Air Surgeon, AAF.

Awards and Grants

Duane Roller, professor and head of the Department of Physics, Wabash College, Crawfordsville, Indiana, will receive the Oersted Medal of the American Association of Physics Teachers at the Association's meeting January 31 at Columbia University. The medal is awarded annually "for notable contributions to the teaching of physics," and in the case of Dr. Roller is a tribute to his work as editor of the *American Journal of Physics* since its foundation in 1933. In his address of acceptance at the meeting Dr. Roller will discuss physical terminology.

Karl Terzaghi, professor of the practice of civil engineering, Harvard University, was awarded the Norman Medal of the American Society of Civil Engineers for the third time January 15. Dr. Terzaghi, who won the medal in 1930 and 1943, went to the Harvard Graduate School of Engineering in 1938 from the Technische Hochschule, Vienna. He was recently appointed professor of the practice of civil engineering (*Science*, January 3).

The American Society for Horticultural Science has awarded the $500 Leonard H. Vaughan Research Award in Horticulture to both Charles M. Rick, University of California, and G. A. L. Mehlquist, Missouri Botanical Garden, formerly of the University of California at Los Angeles. Dr. Rick's paper, "Field Identification of Genetically Male-Sterile Tomato Plants for Use in Producing F_1 Hybrid Seed," and Dr. Melhquist's, "Inheritance in the Carnation. V. Tetraploid Carnations from Interspecific Hybridization," were published in the Society's *Proceedings* for 1945.

The Ella Sachs Plotz Foundation in 1946 made 18 grants to individuals and two to institutions doing research on problems related to medicine and surgery. Since it was founded 23 years ago the Foundation has made 551 grants to scientists throughout the world. Applications for grants in 1947–48 must be in the hands of Dr. Joseph C. Aub, Massachusetts General Hospital, Fruit Street, Boston 14, Massachusetts, U.S.A., before April 15, 1947. Letters asking for aid, which will usually be grants of less than $500, must include qualifications of the investigator, accurate description of the research, size of grant requested, specific use of money to be expended, other sources of support or expected support, and letters of recommendation.

Henry B. Mann, associate professor of mathematics, Ohio State University, was awarded the Frank Nelson Cole Prize in theory of numbers at a meeting of the American Mathematical Society in Swarthmore, Pennsylvania, December 28. The Cole Prize, established in 1928, is awarded each five years to a member of the Society who in the preceding period has published the most outstanding paper on the theory of numbers.

Colleges and Universities

The University of Leiden, Netherlands, has announced the following changes in science professorships since the liberation: new appointments—C. J. Gorter, physics; E. Havinga, organic chemistry; T. H. van den Honert, plant physiology; E. H. Vogelenzang, pharmacy; retirements—J. J. Blanksma, organic chemistry; W. van der Woude, mathematics; W. H. Keesom, physics; E. Hertzsprung, astronomy; resignations—L. G. M. Baas Becking, plant physiology; dismissed—J. J. Lynst Zwikker, pharmacy.

Harvard University has announced promotion of the following faculty members to the rank of full professorship: Lars V. Ahlfors, Garrett Birkhoff, and Saunders MacLane, mathematics; Paul D. Bartlett and E. Bright Wilson, Jr., chemistry; Marland P. Billings and Francis Birch, geology; Lemuel R. Cleveland and Kenneth V. Thimann, biology; Carleton S. Coon, anthropology; and Jabez C. Street, physics.

At the same time James J. Lingane and Robert B. Woodward, chemistry, were advanced to the rank of associate professor.

Twelve additional full professorships in the Faculty of Arts and Sciences were granted in nonscience fields at the recent meeting of the Board of Overseers, bringing the total to 23. Average age of the new professors is 44, six being under

40 years and 12 between the ages of 40 and 45, the University disclosed.

The University of Michigan Board of Regents on December 27 accepted gifts totaling $61,000 for the following purposes: $10,000 from the estate of Gertrude French, Rockford, Illinois, for the Lt. Francis Brown Lowery scholarship in the College of Engineering; $9,400 from the U. S. Public Health Service, through the National Institute of Health, for gastrointestinal research under H. Marvin Pollard; a grant from E. I. du Pont de Nemours & Company, Wilmington, Delaware, to renew the du Pont fellowships in chemistry, chemical engineering, and mechanical engineering for 1947–48; and $4,000 from the Upjohn Company, Kalamazoo, Michigan, for a fellowship in histology in 1947.

Duke University School of Medicine has announced the appointment on January 1 of Eugene A. Stead as professor of medicine to succeed Frederic M. Hanes, who died last March.

The School of Medicine Fungus Disease Registry announces a month's course in mycology to be offered under Norman F. Conant in the summer of 1947. The registry, financed by a grant from the American Foundation for Tropical Medicine, supplies sets of teaching cultures for medical schools and maintains a diagnostic service that includes culture, histopathologic and serologic investigations.

Meetings

The American Oil Chemists' Society will hold its 38th annual meeting May 20–22 in New Orleans, with H. P. Newton, Southern Regional Research Laboratory, general chairman, and A. M. Altschul of the same laboratory, program chairman. The Society has changed the name of its publication to *Journal of the American Oil Chemists' Society* with the January 1947 issue. The name had been *Oil & Soap* since 1932; before that for eight years it was *Journal of the Oil and Fat Industries*; and prior to that, from 1917 to 1924, Society papers were published as the Chemists' Section in the *Cotton Oil Press*.

Elections

Sigma Delta Epsilon, graduate women's scientific organization, at a Boston meeting in connection with meetings of the AAAS elected the following national officers for 1947: Lela V. Barton, Boyce Thompson Institute for Plant Research, president; Winona Welch, DePauw University, 1st vice-president; Pearl Claus, University of Wisconsin, 2nd vice-president; Frances Lloyd Naylor, University of Washington, secretary; and Beulah Armstrong, University of Illinois, treasurer.

The New Hampshire Academy of Science, meeting at Keene State Teachers College, Keene, New Hampshire, October 25–26, elected these officers for 1946–47: Donald H. Chapman, University of New Hampshire, president; Richard H. Goddard, Dartmouth College, vice-president; Richard C. Jones-University of New Hampshire, secretary treasurer; and Hilbert R. Siegler, Department of Fish & Game, Concord, member of the executive council for four years.

The Committee of Food Sanitarians, organization for accumulation and dissemination of information relating to problems of insect and rodent control within the food processing industry, have elected for 1947, E. M. Searles, chairman; E. L. Holmes, vice-chairman; J. P. Barrett, secretary; and G. S. Doolin, treasurer.

Competitive examinations will be held early this year for 75 positions, with grades of assistant and senior assistant scientist, in the regular corps of the U. S. Public Health Service. Written examinations will be held April 14 and 15, and oral examinations during the period February 13–April 9, in bacteriology, mycology, parasitology, entomology, malacology, biology, chemistry, physiology, physics, statistics, and psychology. Salaries are $3,811 and $4,351 for assistant and senior assistant scientists with dependents. Applications may be obtained from the Surgeon General, U. S. Public Health Service, Washington 25, D. C.

Recent Deaths

Johan Wilhelm Sandström, 72, Swedish meteorologist and oceanographer, died in Stockholm January 12.

Rosa Smith Eigenmann, 88, ichthyologist, died in San Diego January 12. Wife of Carl Eigenmann, former dean of the Graduate School and head of the Zoology Department, Indiana University, who died in 1927, Mrs. Eigenmann was first woman president of Sigma Xi.

Charles O'Rourke, 50, professor of structural engineering, Cornell University, died at the Cornell University infirmary January 10 after a long illness.

David R. Morris, 67, meteorologist in charge of the Weather Bureau's New York observatory in Central Park, died of a heart attack at his home January 9. Mr. Morris wrote on air pollution for technical journals, collaborated on studies of allergies to various pollens in the air, and improved apparatus for measuring and analyzing pollution of air by smoke, dust, and pollen.

George Singer, 41, assistant chief, X-ray Section, Bureau of Standards, died suddenly at the Bureau January 16. Mr. Singer worked on the proximity fuse project, developed a method for X-ray inspection of TNT filling in bombs and shells, and during the war organized a large group of users of high-voltage X-ray equipment and then formulated a safety code now standard for the industry in this country.

James W. Lawrie, 65, technical director and director of research, Joseph Schlitz Brewing Company, died January 14 after a two-week illness. Dr. Lawrie in 1937 isolated vitamin K from alfalfa.

Karl Mannheim, 53, lecturer in sociology, London School of Economics, died in London January 9. Born in Hungary, Dr. Mannheim was removed from the chair of sociology at Frankfort University in 1933, at which time he went to Britain. Illness forced him to refuse chairmanship of the European section of UNESCO as well as an offer to reorganize Canberra University in Australia.

Winford P. Larson, 66, head, Department of Bacteriology and Immunology, University of Minnesota since 1918, died in Minneapolis January 1.

Harry Plotz, 55, consultant to the Secretary of War after his retirement last year as head, Virus Division, Army Medical Corps, died in Washington January 6. He worked mainly with the typhus bacillus and mass typhus control, receiving the Legion of Merit in the recent war and foreign and U. S. recognitions in the World War I.

Samuel Gargill Milligan, 83, former professor of physiology, West Penn Medical College, died in Pittsburgh January 4.

Harry E. Barnard, 72, Indiana State Food Commissioner for many years and pioneer in pure food laws, died in Indianapolis December 30.

TECHNICAL PAPERS

The Species Character of Cancer Cells

ROBERT G. GREEN

Department of Bacteriology and Immunology, University of Minnesota Medical School, Minneapolis

In our investigations of the cause of mammary cancer of inbred strains of mice (*11*), immunologic studies suggest that the species character of the cancer cells may be determined by the associated virus rather than by their origin from the tissues of the mouse.

The mouse mammary cancer virus is highly antigenic. Upon injection into rats or rabbits, it stimulates the production of immune bodies (*1*, *8*). An antiserum made from immunized rabbits or rats will not only neutralize the virus but, *in vitro*, will also inactivate mouse mammary cancer cells (*5*). Normal rabbit serum and antiserum from rabbits immunized with normal mouse mammary gland tissue do not neutralize the mouse cancer virus, nor do they inactivate mouse mammary cancer cells. It therefore appears to be the specific immune bodies in the virus antiserum that accomplish both effects. Our further investigations utilizing absorption tests (*6*) show that the immune bodies in the antiserum will combine only with the mammary cancer cells, and that they will not combine with the normal lactating breast tissue cells from which the cancer cells are derived.

There are two indications in our data that point to some extremely close relationship, or combination, of the virus and the cancer cell. The first is that in absorption tests the virus antibodies combine with the cancer cell but do not combine with the normal cell of the same kind. While this finding demonstrates a specific absorption of immune bodies by the cancer cell, the absorption could be due to free virus contained within it. The second is that the virus immune bodies have an inactivating, or lethal, effect upon the cancer cell, which would seem to mean that some virus is not free but occupies a vital position in the cancer cell. The malignant animal cell seems to have become completely dependent upon the vital activity of the virus associated with it. The two findings together suggest not only that the virus is intimately associated with the cancer cell but also that the association is concerned with the species character of this cell, since these immunologic reactions are ordinarily species specific. Immunologically, the mouse mammary cancer cell appears to be a mouse cell with a substituted virus species.

These immunologic findings for the mouse cancer do not stand alone. Those of Kidd and Rous (*10*) for the Shope virus-cancer complex and of Kidd (*9*) for the Brown-Pearce cancer of rabbits fall into the same general pattern. The experimental results so far available for these two rabbit cancers can be interpreted to mean that in each case an antigen foreign to the normal rabbit cell is present in some close combination in the cancer cell. The antigens are foreign to the rabbit species, since in both cases the rabbit cancer cell stimulates antibody formation in the rabbit, which is the same animal species from which the cancer cell was derived. The rabbit cancer developing from the Shope papilloma does not appear to contain free virus, but upon its transplantation to normal rabbits the rabbits become immune to the Shope papilloma virus as the cancer transplant grows. Here the cancer cell is antigenic, and the species character of the cancer cell appears to be that of the associated Shope virus. In the case of the Brown-Pearce cancer, the foreign antigen associated with it is not known to be a virus. However, the foreign antigen is separable from the cancer cell and upon injection into rabbits produces a specific antiserum. Since, as Kidd (*9*) has recently shown, the antiserum injected into rabbits afflicted with the cancer has some effect on regression of the cancer, the immune bodies appear to combine specifically with the cancer cells, as we have demonstrated for the mouse mammary carcinoma. Thus, the results of immunologic studies on three animal tumors suggest that the species character of the cancer cell is not that of the animal of origin but that of an associated virus.

It appears to be biologically sound to consider viruses as simplified forms of microbes which have developed their small size and physiological dependencies as retrograde changes due to intracellular parasitism (*2*, *4*). It seems logical, also, to conceive that an end result of the retrograde parasitic process could be an actual mergence, or hybridization (*3*), of the parasitic virus with the host cell. The data which we have obtained on the mouse mammary cancer definitely point to some sort of intimate combination, or hybridization, of the virus with the mouse mammary cell. It would seem that a hybridization resulting in a change in the species character of the cell might well result in a new type of parasitic cell (*7*), which would grow as a cancerous process quite independent of control by the animal organization of which it was formerly an integral part. A similar basic change in the species character of a cell might also be produced by coal-tar chemicals or by radiant energy, and these changes could also result in cancerous growth, since the species character modification would likewise remove the cell from the corporate growth control. From the standpoint of modification or substitution of the species character as a basic cause of cancerous growth of cells, the mechanism of cancer production by coal tar, by radiation, and by a virus seems to be basically and essentially the same process.

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New Concept of Competitive Inhibition of the Renal Tubular Excretion of Penicillin

KARL H. BEYER,[1] *Department of Pharmacology, Sharp & Dohme, Inc., Glenolden, Pennsylvania*

It has been established that penicillin is excreted by way of the renal tubules in addition to glomerular filtration (*1*). Its over-all clearance at all plasma concentrations studied to date appears to be equivalent to renal plasma flow, as measured directly or by p-aminohippuric acid, the extraction ratio of which is reported to be 0.88:0.93 (*9*). Where a normal relationship exists between glomerular filtration and renal plasma flow, the fraction of penicillin filtered at the glomeruli represents in man about 20 per cent of the total amount excreted per unit time. Conversely, at customary blood levels, about 80 per cent of the penicillin excreted is eliminated by way of the renal tubules. Thus, the suppression of penicillin excretion by the tubules could be expected to effect a considerably higher plasma concentration for a longer period of time than normally would obtain following a given dose of penicillin.

This reversible inhibition of the excretion of penicillin by the tubular epithelium has been effected by the administration of diodrast (*8*) or PAH (p-aminohippurate) (*2*). These compounds are excreted by the same tubular transport mechanism and as rapidly as penicillin. Hence, it is possible, by maintaining a high plasma concentration of either compound, to saturate functionally that mechanism and thus suppress the tubular elimination of penicillin on what loosely may be considered a "mass action" basis. This principle has been applied successfully in therapy (*5*), but the very great amount of PAH needed to produce this effect seriously limits its applicability from a practical standpoint.

Acknowledging these limitations requiring intravenous administration and very large dosages of PAH with penicillin, a new approach was sought which resulted in the concept involving the application of the compound described herein. This compound appears to combine the advantages of high and sustained blood levels of penicillin administered orally or parenterally at dosages that are feasible.

The purpose of this report is to present briefly the broad aspects of this concept without detailed qualification or substantiation of each point. Laboratory and clinical substantiation will be submitted in detail in a forthcoming series of publications.

Excretion by the renal tubules is a remarkably selective process. The transport mechanism for the excretion of penicillin, like other differentiating mechanisms for glucose and amino acid reabsorption, is dependent on the viability of the tubular epithelium. Although without precedent, it seemed desirable to inhibit reversibly and selectively the transport mechanism for the excretion of penicillin without necessarily abolishing other transport systems or impairing the vitality of the cell as a whole. If this could be done, the remaining pathway for penicillin excretion would be glomerular filtration, which would represent only about 20 per cent of the normal rate of penicillin elimination.

While in effect there may be a number of such seemingly discrete systems whose functional orientations are directionally opposite within the cell (excretion-absorption), the multiple components of a single transport mechanism need not be unique, either descriptively or functionally, to that system that gains singularity only through some definitive component or components. Since most metabolic processes are enzymatic, it seemed possible to inhibit selectively such a fundamental or definitive enzymatic component of an otherwise complex process and so halt the particular system involved in the excretion of penicillin.

The competitive inhibition of the enzymatic alteration of one substrate by another which has an affinity for, but is refractory to, the action of an enzyme or enzyme system occurs *in vitro*. Examples of such *in vitro* substrate competition are the inhibition by malonate of succinate oxidation by the succinoxidase system (*7*), and the inhibition by ephedrine of tyramine deamination by amine oxidase (*3*). Moreover, the affinity of the competitive substrates appears to be for the definitive enzyme, succinic dehydrogenase (*6*) or amine oxidase (*4*), in each complex system. These are reversible inhibitions wherein the integrity of the enzyme system has not been violated inalterably.

TABLE 1

PROTOCOL OF AN EXPERIMENT ILLUSTRATING THE INHIBITORY EFFECT OF 4'-CARBOXYPHENYLMETHANESULFONANILIDE ON PENICILLIN CLEARANCE

Time (hr.:min.)	Urine vol. (cc./min.)	Penicillin		Creatinine clearance (cc./min.)	Filtration fraction
		Plasma conc. (units/cc.)	Clearance (cc./min.)		
Dog 240					
Control phase	*Penicillin—Priming* dose: 15,200 units, i.v. Maintenance dose: 156 units/min. in 5% glucose, 3 cc./min., i.v.				
0:35	3.1	0.68	278.1	75.2	0.27
0:45	3.3	0.58	212.8	63.6	0.30
Drug phase	*Penicillin*—Maintenance dose: 136.5 units/min. at 3 cc./min., i.v. *Drug*—Priming dose: 25 mg./kg., i.v. Maintenance dose: 30 mg./kg./hr., i.v.				
1:30	3.5	0.79	51.9	60.3	1.16
1:40	3.6	0.83	57.2	59.5	1.04

Transposing into terms of renal physiology, if it were possible to find a compound which would inhibit the excretion of an agent by the tubules on the basis of specific enzymatic substrate competition, it should have the following essential chemical and pharmacodynamic characteristics: (1) It should have an affinity for the particular enzyme system that characterizes the selectivity of the over-all tubular mechanism for penicillin excretion. (2) If it is sufficiently different to be refractory to the action of that system, its renal elimination should be limited essentially to glomerular filtration. (3) The inhibition of penicillin excretion should be maximal at relatively low plasma concentrations, as compared to the type mass action effect induced by PAH. (4) It should not alter the function of other transport mechanisms either selectively

[1] The data presented here were obtained through the cooperation of Drs. W. F. Verwey and A. K. Miller, of the Department of Bacteriology, and Mr. H. F. Russo and Miss E. A. Patch, of the Department of Pharmacology.

or collectively at plasma concentrations sufficient to inhibit maximally the tubular excretion of penicillin. Effects produced at excessive elevations of blood levels of the compound sufficient to be toxic in the customary sense of the word are, of course, beyond the intent of this principle. (5) If the agent acts by competitive inhibition of an enzymatic reaction, in accordance with this concept, the process should be reversible. (6) Such a compound should not necessarily influence either renal blood flow or glomerular filtration rate. However, it would vitiate the use of PAH for the measurement of renal plasma flow or normal PAH_{Tm}, for PAH is excreted in a manner similar to that for penicillin. (7) The agent should not have a high order of systemic toxicity. (8) The compound need not necessarily influence any properties peculiar to penicillin, such as its bacteriostatic action, inactivation, etc. (9) It follows from (2) and (3) that the dosage of the compound would be quite practical. (10) It was anticipated that these properties might be contained in a compound or compounds having the additional advantage of oral efficacy at reasonable dosage.

Of the compounds synthesized for this research by the Department of Organic Chemistry of these Laboratories, 4′-carboxyphenylmethanesulfonanilide incorporates essentially the properties listed above.

In the experiment summarized in Table 1, penicillin was infused at a rate and in an amount that would permit a falling penicillin plasma concentration if the drug were not effective. Duplicate control penicillin and creatinine clearances were obtained. The drug then was injected as a priming and maintenance dose, its distribution in the body allowed to equilibrate, and additional penicillin clearances obtained. It may be seen that the tubular excretion of penicillin was completely suppressed, as indicated by the rising penicillin plasma concentration, decreased penicillin clearance, and increased filtration fraction.

Fig. 1 illustrates two experiments wherein dogs were administered 100,000 units of penicillin by stomach tube every 4 hours for 16 hours, including the 4-hour period wherein the control curves for penicillin plasma concentration were obtained. Immediately after the 4-hour control blood samples were taken, each dog was given 100,000 units of penicillin plus 60 mg./kg. of the drug by stomach tube, another curve for plasma concentration of penicillin being obtained over the next 4-hour period. It may be seen that in the control phase the maximal penicillin plasma concentrations were 0.8 and 1.2 units/cc. and at 2 hours the values were less than the lower limits of the Florey cup-plate assay, 0.24 units/cc. Following administration of drug and penicillin the maximal penicillin plasma concentrations were 2.85 and 3.6 units/cc. In one instance the peak occurred at 2 hours following oral administration. At 4 hours the plasma concentration was still somewhat above 0.5 units/cc.

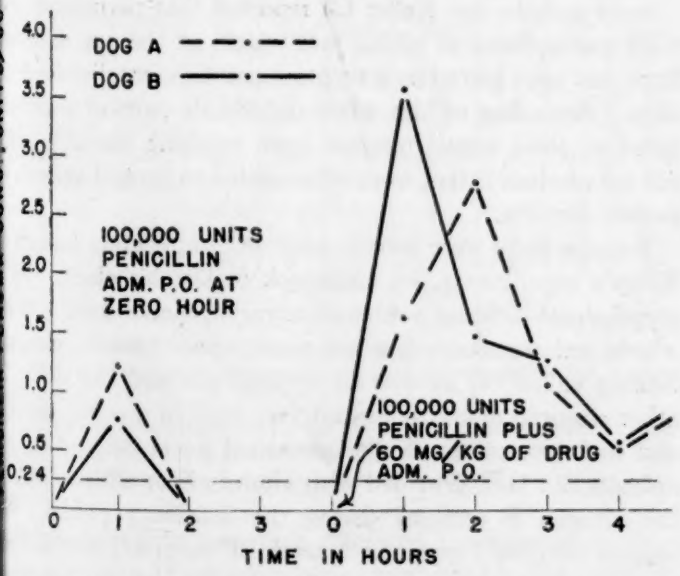

FIG. 1. The blood level response to equivalent oral dosages of penicillin alone and with the coadministration of 4′-carboxyphenylmethanesulfonanilide.

Summary. It has been found that the excretion of penicillin by a renal tubular transport mechanism could be physiologically inhibited reversibly. The basis for this effect is thought to be one of substrate competition between penicillin, which is excreted by the tubules, and 4′-carboxyphenylmethanesulfonanilide, which is essentially refractory to excretion by that transport mechanism.

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Some Effects of O-Isopropyl N-Phenyl Carbamate Upon Cereals[1]

W. B. ENNIS, JR.

Camp Detrick, Frederick, Maryland

Most of the reports in the literature concerning the use of chemicals as plant-growth regulators indicate that application of such substances in some form to the aerial portions of plants is the most common practice. In the instance of O-isopropyl N-phenyl carbamate, however, greatest success appears to have been achieved through applications to the soil in a suitable diluent or carrier, and the results of applications in sprays to the plant itself have been inconclusive (*1*, *2*). To test further the herbicidal activity of sprays of this compound, several experiments have been carried out using oats and barley as test plants.

To insure thorough wetting of the leaves, young oat and barley plants were exposed by immersion of the aerial plant portions for intervals of 2–4 seconds up to 10 minutes in isopropyl alcohol-water solutions containing 500, 250, or 100

[1] These studies were conducted from March 1946 to August 1946 under the supervision of Dr. C. E. Minarik.

ppm of O-isopropyl N-phenyl carbamate and a wetting agent. Care was exercised to prevent contamination of the soil by the growth regulator. The subsequent plant growth appeared normal, and there was no reduction in yield of grain. These results indicated that O-isopropyl N-phenyl carbamate does not influence the growth of such plants when applied to the

TABLE 1

EFFECT UPON OATS OF EXPOSING SOIL TO SPRAYS OF O-ISOPROPYL N-PHENYL CARBAMATE

Rate of O-isopropyl N-phenyl carbamate in grams/sq. yd.	Mean grain yield per pot (grams)	
	Soil exposed	Soil not exposed
0.5	0.55	4.00
1.5	0.04	2.31
0.0 (10% TBP*)	4.25	—
0.0 (30% TBP*)	4.05	—
0.0	6.05	—

Minimum significant difference between means at the 5 per cent level of probability is 2.0 grams; at the 1 per cent level, 2.68 grams.

* 10% or 30% tributyl phosphate in oil applied in same proportion as in the 0.5-and 1.5-gram rates of O-isopropyl N-phenyl carbamate, respectively.

leaves at these concentrations. In order to establish that these preparations possessed herbicidal activity, applications were made to the soil of similar plants, each application being equivalent to 5 mg. of compound/4-inch pot. This treatment was lethal.

To test the effects of sprays of this compound when allowed to contaminate the soil, potted oat plants approximately 15 inches in height were sprayed in quadruplicate with O-isopropyl N-phenyl carbamate in an oil spray. Tributyl phosphate was used as a cosolvent at the rate of 2 ml./gram of the growth regulator. The desired concentrations were made by diluting with No. 2 fuel oil. The rates of application were 0.5 and 1.5 gram/square yard in 10 ml. of solution. The plant-growth regulator was applied to plants at each rate, the soil being

TABLE 2

EFFECT UPON OATS OF EXPOSING SOIL TO SPRAYS OF O-ISOPROPYL N-PHENYL CARBAMATE

Treatment	Mean grain yield per pot (grams)	
	Soil exposed	Soil not exposed
O-isopropyl N-phenyl carbamate	0.63	1.45
Acetone-water (1:2) control	1.18	1.26
Untreated control	1.51	1.51

Minimum significant difference between means at the 5 per cent level of probability is 0.36 gram; at the 1 per cent level, 0.48 gram.

protected from the spray in one instance and exposed in another. It is to be noted (Table 1) that the yield of grain from the plants where the soil was exposed to the spray was significantly less than that from the corresponding protected series. This suggests that much of the effect from such sprays of this substance results from soil contamination.

Since considerable injury to the plants was caused by tributyl phosphate in the above spray, a further experiment was carried out using acetone as a cosolvent. Oats 12–15 inches in height were sprayed with acetone-water (1:2) solutions of O-isopropyl N-phenyl carbamate with the soil protected from the spray in one case and exposed in another. The carbamate was applied at the rate of 0.5 gram/square yard in 20 ml. of solution. The plants making up the series in which the soil was protected from the spray were not noticeably affected, whereas the series having the soil exposed to the spray yielded highly significantly less grain (Table 2).

Practical implications of these findings are that spraying O-isopropyl N-phenyl carbamate may not be the most efficient method of application, since in these studies inhibitory effects were not induced in young oat and barley plants by exposing the aerial plant portions to sprays or solutions of this compound. Should sprays be used for application of this plant-growth regulator as a herbicide, it appears that emphasis should possibly be directed toward actual soil treatment. It is suggested that this may be achieved by applying the carbamate with a suitable diluent or as an impregnated material. Since it has been reported (*1*) that young cereals are more susceptible to O-isopropyl N-phenyl carbamate than older plants, it is further suggested that for most efficient use it should be applied when the plants are in a young stage of development, or even prior to seedling emergence.

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Is Sterility Induced in Growing Rats on a Tryptophane-deficient Diet?

CLARENCE P. BERG and WAYNE G. ROHSE

State University of Iowa, Iowa City

Some months ago Keller (*3*) reported that permanent sterility was induced in albino rats which, at the age of 28–48 days, had been placed on a tryptophane-deficient diet for 3–18 days. According to him, male and female pairs of such rats failed to show sexual interest upon reaching maturity, and did not produce litters, even when mated to control animals of proven fertility.

Because there were several possible complicating factors in Keller's experiments, we undertook to test his claim. As a tryptophane-deficient source of nitrogen, Keller used gelatin, which, unfortunately, is a notoriously poor protein, possibly lacking valine (*4*) as well as tryptophane and being low in other essential amino acids and very high in glycine, proline, and hydroxyproline. Keller presented no evidence that his animals had been provided with vitamin E or with factors of the vitamin B complex during the deficiency period. We assume that the 5 grams of "castor oil" recorded as a dietary component was an error and that cod-liver oil was meant.

In planning our tests we tried to avoid as many complicating factors as possible. Three types of tryptophane-deficient diets were used. In one (G), bacto-gelatin served as the sole source of amino-acid nitrogen; in a second (Hy), acid-hydrolyzed casein (*1*) was employed; and in the third (Pr-free), there was no protein. The compositions of these diets, the control diets (Gt, Gvt, Hyt), and the stock diet are recorded in Table 1. A synthetic mixture of the factors associated with the vitamin

B complex was fed separately twice daily to each rat except those maintained on the stock diet, which provided an ample natural supply in the yeast. Each vitamin allotment was fed in the form of a pellet containing thiamine, 20 μg.; riboflavin, 30 μg.; and pyridoxine, 30 μg.; calcium pantothenate, 0.1 mg.; nicotinic acid, 0.5 mg.; p-aminobenzoic acid, 1.0 mg.; inositol, 1.0 mg., choline, 10 mg., starch, 50 mg., and corn syrup, 25 mg. Crisco supplied vitamin E.

At 26 days of age, 19 male and 19 female rats of the Sprague-

TABLE 1

COMPOSITION OF TRYPTOPHANE-DEFICIENT, CONTROL, AND STOCK DIETS*

	G (grams)	Gt (grams)	Gvt (grams)	Hy (grams)	Hyt (grams)	Pr-free (grams)	Stock (grams)
Bacto-gelatin	30.0	29.8	28.4				
Tryptophane-deficient casein hydrolysate				17.7	17.5		
Casein							17.7
l-Cystine				0.3	0.3		0.3
l-Tryptophane		0.2	0.2		0.2		
dl-Valine			1.4				
Starch	25.0	25.0	25.0	37.0	37.0	55.0	29.0
Yeast							8.0

* Diets other than the stock diet are designated by symbols which suggest their nitrogen composition: G, gelatin; Hy, casein hydrolysate; t, supplementation with tryptophane and v, with valine; Pr-free, protein free. Per 100 grams, each diet also contained glucose, 15 grams; Crisco, 19; cod-liver oil, 5; salt mixture (*2*), 4; and agar, 2. The B-complex vitamins were provided in pellets as described in the text.

Dawley strain were placed on the diets outlined, the same number of males as of females on each. After 20 days (2 days longer than the longest period of Keller), all of the animals

TABLE 2

AVERAGE GROWTH DURING THE 20-DAY PERIOD ON THE VARIOUS DIETS TOGETHER WITH SUBSEQUENT REPRODUCTION HISTORY

Diet fed during the 20-day period	G	Gt	Gvt	Hy	Hyt	Pr-free	Stock
Pairs of rats tested	4	3	2	4	2	2	2
Avg. initial wt. per rat (grams)	43.1	42.0	41.5	43.7	45.0	47.0	43.0
Avg. change in wt. per rat (grams)	−13.6*	0.0	+1.0	−11.7	+44.0	−12.0	+79.0
Size of litters†	11, 13	10	6	10, 7, 8	14	9	11
	10, 11	9	6	8, 11	10	8	8
	5, 7	8		7, 14			
				6, 10			

* One of the females on the unsupplemented bacto-gelatin diet died on the 20th day.

† One of the females that had been on the tryptophane-deficient diet, Hy, was mated three times and bore three litters; all others that had been on diets G and Hy were mated twice and bore two litters.

were transferred to the stock diet, and the separate feeding of the synthetic vitamin B complex mixture was discontinued. At 100 days of age, male and female rats which had been given the same diet during the initial 20-day period were paired off for mating. To make sure that the supply of vitamin E was adequate for implantation and gestation, 15 grams of wheat-germ oil were added to each kilogram of stock diet. Table 2 summarizes the observations made on growth in the 20-day deprivation period and indicates the number of young in the litters resulting from each mating.

During the 20 days on the experimental diets, the animals on the tryptophane-deficient hydrolysate (diet Hy) lost about one-fourth of their initial weights. The fact that this loss was essentially the same as that in the rats on the protein-free diet (Pr-free) indicates that the casein hydrolysate was markedly deficient; stimulation of growth by the incorporation of tryptophane (diet Hyt) affords evidence that the hydrolysate lacked chiefly this amino acid (*1*). The weight loss on the unsupplemented gelatin diet (G) was slightly, but possibly not significantly, greater than on the other tryptophane-deficient diets. Supplementation of gelatin with tryptophane (diet Gt) or with both tryptophane and valine (diet Gvt) prevented the loss in weight but did not promote significant growth in the 20 days. This supports our earlier implication that it is quite unfair to assume that unfavorable responses on gelatin diets may be attributed, without question, to their deficiency in tryptophane.

The reproductive performance of the pairs of rats which had been fed the tryptophane-deficient diets (G, Hy, and Pr-free) compared very favorably in every way with that of the control pairs fed supplements of tryptophane (Gt, Gvt, and Hyt), as well as with that of the control pairs fed the stock diet throughout.

We are not prepared to account for the observations reported by Keller, but the tests recorded in this communication convince us that a period of *uncomplicated* tryptophane deficiency in young rats, lasting as long as 20 days, *does not induce subsequent sterility.*

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Cypress Domes

ROBERT O. VERNON

Florida Geological Survey, Tallahassee

The extensive flatlands of the peninsular area are a monotonous part of the Florida landscape, characterless except for vegetation growths. One of the most unique of these are large stands of cypress that appear, at a distance, to be buttes standing out against the horizon (Fig. 1). These stands of cypress occur in dome-like masses, with the taller trees growing in the center and those toward the periphery being successively shorter (Fig. 2). Some of these groves are of immense proportions, being up to a mile in diameter. The map appearance of the dome may be round, elliptical, or, in rare cases, there may be open water in the center, the dome appearing as a doughnut. Both species of cypress are present in these stands, *Taxodium distichum* (L.) Richard, for example, growing along the St. Johns River Valley, and *T. ascendens* Brongn, in the Big Cypress of the southern Peninsula. The uniform occurrence of these domes throughout the Peninsula of Florida suggests a close relationship to the geomorphology of Florida and is therefore of interest to others than the botanist.

These cypress domes were discussed by Harper (*3*), who thought that the trees nearest the periphery were dwarfed by unfavorable soil conditions and believed that the small trees were as old as the large. Kurz (*4*) proved, by counting the tree rings, that the trees were progressively younger toward the outside of the dome. He explained the dome shape by an increasing premature mortality rate in the cypress from the center of the dome to the outside. He was unable to explain just why cypress "should die back partly or altogether prematurely in the shallower water where growing conditions should be best." Likewise, a high mortality rate progressively nearer the periphery does not explain why older trees are not scattered throughout the dome, as would be expected.

FIG. 1. Cypress domes on prairie near Deer Park, west of Holopau, Florida. (Photograph taken by John H. Davis, Jr., from highway U. S. 192.)

Plant ecologists have proved that cypress seed will not germinate when covered with water, and Demaree (*2*) showed that, if it is to sprout, the seed must lie on land that is not submerged, and the seedling, to survive, must grow enough to keep above seasonal water rises. Demaree cites the case of Reelfoot Lake in Tennessee, the basin of which was suddenly created by the New Madrid earthquake of 1811–12, as proof that even old and established cypress trees will be drowned if they are rapidly submerged in water. Experiments conducted by him in 1928–29 on the effect of drowning on cypress seed and seedling support his Reelfoot Lake observations, that a growing cypress tree or seedling will die if it is suddenly submerged in water. He did not attempt to study the effect of gradual drowning on cypress, but observations made by the writer in 1943 indicate that, where submergence is slow and gradual, the cypress tree is able to adjust, in moderation, to deeper water, even where this water becomes permanent. In western Florida, where the more rapid alluviation in main valleys has caused the ponding of tributary streams and the formation of lakes in the lower parts of the tributary valleys, the trees growing on the flood plains and marginal swamps were slowly drowned (*5*). These lakes mark an advancing regional filling of previously eroded valleys, and the ponding dates back to the start of the Recent and is indicative of a rising sea level that has initiated valley filling. Thus, ponding and subsequent drowning of the trees growing on the flood plains of the tributary streams was very gradual, and the cypress occurring in these lakes record the reaction of cypress to slow drowning. Dead Lake, the drowned lower portion of the Chipola River, is studded with cypress trees and stumps and is an excellent illustration. In a line starting at the Chipola River channel, near the center of the lake, and running to the lake banks, the general succession of tree vegetation is (1) partially submerged and rotted stumps, (2) partially rotted stumps extending above water, (3) decayed trees still retaining dead branches and an occasional live branch, (4) vigorous mature growth, (5) saplings, and (6) seedlings (Fig. 3). The stumps near the channel have buttresses that extend above the present lake level, and some of these now stand in 25 feet of water. The cypresses have thus been able to adjust to, and build buttresses in, water depths of at least 25 feet before being killed, and the succession of development of trees indicates that new trees have worked continuously toward the banks of a slowly spreading and deepening lake.

FIG. 3. Succession of cypress growth in the south end of Dead Lake, along the lower part of the Chipola River, Gulf County, Florida. (Photograph taken from the channel.)

FIG. 2. Bald cypress (*T. distichum*) dome west of Melbourne, Florida. (Photograph taken by Herman Kurz.)

Stands of cypress where the tallest trees grow in the center and shorter trees grow outside are called domes because of the cross-section profile when viewed from the side. Where an elongated growth of cypress with linear dimensions ends in a dome, the dome is referred to as a cypress head, and if open water is present in the center the growth is called a cypress doughnut.

The observations made in Dead Lake and on the cypress domes of the Peninsula of Florida have led the writer to believe that these domes were started in seasonal ponds or swamps, where the seed sprouted when not submerged, the seedling then growing sufficiently high to stay above seasonal rises in water level. The subsequent development of the dome is most easily explained by a gradually deepening pond, the basin filling so slowly that the biologic changes, necessary for the cypress to adjust to the changing habitat, could keep pace. In such a seasonal pond cypress trees developed and grew and

then spread laterally into the growing pond, the seed floating into the wet area along the pond banks where germination was assured. New growth could not start in the center of the pond, now permanently covered by water. Progressive growths of seedling, sapling, and tree from the periphery toward the center of the pond result in a dome-shaped profile. Where cypress growth started about a permanent pond that was gradually deepening, a cypress doughnut with open water in the center may be formed. Cypresses developing along any natural drainage that ends in a depression as described above will result in the development of a cypress head.

In ponds not connected with artesian aquifers there are three possible causes of the gradual deepening of the water. The basin could be increasing slowly by solution, the basin could be filling with sediment possibly accompanied by subsidence, and the ground-water level could be rising. The uniform development and occurrence of these cypress growths throughout the Peninsula of Florida favors a regional rise of ground-water level and tends to eliminate the other two listed causes, although these may be of local importance. The ground-water surface controlling the water level in these ponds is not artesian, and, as seasonal rises due to heavy rainfall are not permanent, the gradual rise of water level and regional deepening of these ponds and subsequent development of cypress domes, heads, and doughnuts is indicative of a steady rise in sea level throughout the Recent. Because the ground-water surface tends to conform to topography generally, the total rise in ground-water level was proportionally small in relation to the total rise in sea level, and the pond basins filled very slowly.

That the sea level has risen during the Recent has been reported by Davis (*1*), who has described the occurrence of mangrove peats, frequently 10 feet and more in thickness, in the coastal areas of southwestern Florida swamps. Because mangrove growth is limited to within a few inches of the range of the tide, he concluded that these deposits show a rise in sea level, since the present tide range is only three to four feet. Furthermore, in a manuscript describing Florida peat deposits, Davis points out that many lakes contain fibrous peat below a sedimentary clastic peat. As fibrous peat is developed in shallow water marshes and sedimentary peat in deeper water conditions, the water level in the lakes must have risen since the formation of the fibrous peat. This, in turn, could be indicative of a rise in sea level, although other causes must be considered. Such studies as these present a mounting mass of evidence that sea level changes during the Pleistocene and Recent have largely controlled the geomorphology of Florida and adjacent coastal areas. Without considering sea-level changes, it would be difficult, if not impossible, to explain the present physiography, hydrology, soils, and vegetation.

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IN THE LABORATORY

The Concentration and Preservation of Urinary Substances by Lyophilization

Harry B. Friedgood, Arie J. Haagen-Smit, Josephine B. Garst, and Lotti Steinitz

Cancer Research Foundation of California; William G. Kerckhoff Laboratories of Biological Science, California Institute of Technology; and *Department of Medicine, University of Southern California Medical School*

The usual method for concentrating or drying urine is vacuum distillation at elevated temperatures. Its use leaves one in doubt, however, concerning possible changes that may have occurred in the chemical composition of the urinary constituents. Furthermore, the foaming of the urine, resulting from the reduced pressure, interferes with the distillation.

The steroid content of normal urine is relatively low as compared with the amount necessary for its quantitative study. To obviate the difficulties inherent in vacuum distillation, it was thought advisable to investigate the possibility of subjecting large volumes of urine to concentration by lyophilization in order to obtain adequate quantities of steroid substances for analysis.

Urine samples of different concentrations were dried in a lyophil of the type shown in Fig. 1.[1] The lyophil apparatus used in this study was constructed with four equally spaced joints near the base of the jacket of the condenser cone. These joints, placed at a downward angle, were of the short, standard taper 34/45 female type. The male portion of the joints was attached to an 800-cc. Kjeldahl flask. The diffusion path of the water vapor was kept as short as possible.

Not more than 100 cc. of urine in the case of the dilute samples, and proportionally less for the more concentrated ones, are put into each flask of the lyophil. This is frozen in the form of a thin, uniform shell by slowly revolving the flasks in a freezing bath of methyl cellosolve and dry ice. This bath is best contained in an ordinary enameled or agateware saucepan about 7 inches in diameter and 4 inches deep. The freezing is accomplished more uniformly if the bath is cooled substantially before the freezing is started. As soon as the contents of the flask are frozen solid, *i.e.* until the shell of frozen urine cracks or snaps, the flask is transferred to a tray containing more of the methyl cellosolve and dry ice, since all of the flasks must be put on the lyophil in rapid succession. A convenient tray for this purpose is an enameled one approximately 9 x 13 x 2 inches. This will accommodate four of the 800-cc.

[1] Diagram reproduced from *Science*, 1944, **99**, 285–286, by permission of the authors, D. H. Campbell and David Pressman.

flasks. A vacuum of 0.1–0.2 cm. is maintained in the lyophil by means of a Wegner vacuum pump.

As soon as the samples are frozen, dry ice is added to the methyl cellosolve in the condenser cone of the lyophil. The ice is added cautiously in order to obviate the excessive boiling which is otherwise encountered. No more ice than can be

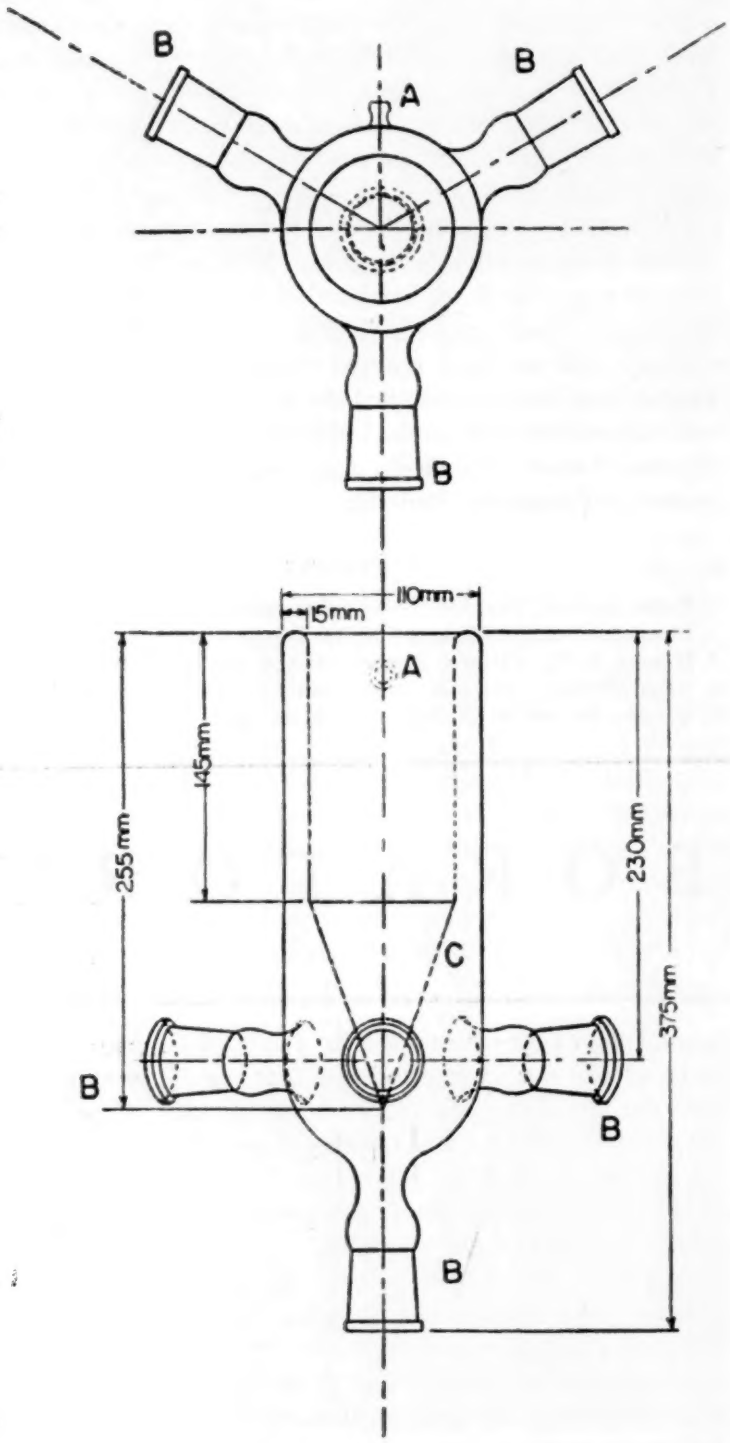

FIG. 1. Diagram of lyophil apparatus showing the side and cross-section views.

covered by the cellosolve is used at this stage, but the mixture is permitted to become cold before the flasks are put on. The ground-glass joints are well lubricated with a light vacuum grease.[2] The flasks are then placed quickly on the lyophil. Care is taken, however, to observe that each joint becomes transparent in order to be certain of good contact. A sealed-off standard taper is put in place on the joint at the bottom of the apparatus before the freezing is started. During the first 20–30 minutes the lyophil is watched rather carefully until the ice or frost begins to accumulate on the condenser cone. This occurs soon after the peak of foaming in the flasks is reached. Although the condenser cone must be kept as cold as possible during this time, it should not contain too much dry ice because considerable boiling occurs when the condensation begins. After the onset of condensation, the condenser cone should be kept full of dry ice.

Once the ice has formed on the outside of the flasks, the apparatus needs attention only at intervals of approximately one hour. The time of drying depends for the most part on the following factors: (1) size of the sample; (2) concentration of the sample; and (3) temperature and relative humidity of the room. The following results were obtained with urine taken to dryness:

Total volume of samples (cc.)	Specific gravity	Time of drying (hrs.)
100	1.095	9½
200	1.016	8
400	1.014	10
400	1.006	8
400	1.025	9

This method of concentrating or drying urine of varying concentrations is satisfactory from a number of standpoints: (1) The apparatus is simple and of moderate cost; (2) the process requires little attention once the initial steps are taken; (3) concentration of the urine is done at below-zero temperatures, which are unfavorable to chemical alterations in the constituents of the urine.

The application of the method to the extraction of steroids is now being studied.

Elementary Isotopic Analysis: Determination of Carbon

A. V. GROSSE, A. D. KIRSHENBAUM, and S. G. HINDIN

Houdry Process Corporation, Marcus Hook, Pennsylvania

We have recently described a new method for the determination of oxygen (*1*) in organic compounds by using heavy oxygen. Using the same method, higher precision is possible in the determination of carbon, since C^{13} is now available (from our two thermal diffusion plants in Marcus Hook) in much higher concentration (10–15 per cent) than the oxygen isotope (1.2 per cent).

In order to demonstrate the increased precision obtainable with our method when using higher isotope concentrations and to extend this method to carbon, we have made the following three carbon determinations on two representative pure compounds, namely, acetic acid (m.p. = 16° C.) and iso-octane (n_{20}^{D} = 1.3916). The $C^{13}O_2$ used had a concentration of 12 atom per cent C^{13}.

A known weight of substance (= a) was burned in a platinum tube with a slight excess of O_2, as described previously (*1*), and simultaneously equilibrated for 30–45 minutes with a known volume of pure isotopic CO_2 (= b). The acetic acid was

[2] Celvacene-Light Vacuum Grease, Distillation Products, Inc., Rochester, New York.

burned at about 500° C., the iso-octane at 750° C. The per cent carbon was calculated from the formula: $x = 100\ b(m - n)/an$, where m and n are, respectively, the initial and final atomic per cent *excess* of C^{13} in the CO_2.

The results were as follows:

	Acetic acid		Iso-octane (C_8H_{18})
	Preliminary run	Final run	
a, mg. substance	20.47	24.04	20.96
b, mg. C, added as CO_2 (using atomic wt. of C = 12.01)	7.90	11.76	11.10
m, % excess of C^{13} above normal (= 1.04%) in b	11.18	11.20	11.24
$C^{13}O_2$-content of equilibrium mixture	6.39	7.19_4	5.37_6
	6.41	7.19_6	5.40_6
	6.36	7.20_8	5.37_8
Average	6.39 ±0.02	7.19_7 ±.006	5.38_1 ±.012
n, avg. % excess of C^{13} in equilibrium mixt.	5.35	6.15_7	4.34_1
% C in substance, experimental	42.1	40.07	83.50
% C, theoretical	40.00	40.00	84.19

A mass spectrograph constructed by the Consolidated Engineering Corporation (Pasadena, California) was used. The average precision obtainable is ±0.3 per cent, based on the carbon content.

A detailed description of this work will be published elsewhere.

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Immobilizing Paramecia for Study in the Introductory Course[1]

Karl A. Stiles and Dorothy A. Hawkins

Michigan State College, East Lansing, and *Coe College, Cedar Rapids, Iowa*

A laboratory study of paramecia for the biology student may be highly instructive and of special motivational value if it comes early in the course. Because of its large size and clear organelles, the paramecium has long been a standard form for the study of protozoa in introductory courses. However, its constant, rapid locomotory activity is often discouraging to elementary students in their efforts to study its structure. Therefore, many methods have been suggested for its immobilization, most of which involve either (1) mechanical restraint, such as entanglement in cotton or lens-paper fibers, (2) immersion in some viscous solution such as agar, gelatin, tragacanth, and quince-seed jelly, or (3) narcosis with chloretone, isopropyl alcohol, and nickel chloride. The first method merely impedes locomotion; the other two either are unsatisfactory immobilizers or interfere with the normal physiology of the animal.

[1] Contribution No. 2 of the Department of Biological Science.

However, immobilizing paramecia has been made relatively easy in recent years with Methocel[2] (methyl cellulose), suggested by Marsland (*3*), or polyvinyl alcohol, described by Moment (*4*). The authors have made comparative studies of these two agents for quieting paramecia and prefer the former, chiefly for the reason that some difficulty was experienced in obtaining clear, homogeneous solutions of polyvinyl alcohol, whereas uniformly good success was enjoyed with Methocel.

Doubtless, Methocel has not been used as generally as it deserves to be by biology instructors, for during World War II it was available only for essential uses. Now, however, it is obtainable for all purposes.

Our technique differs somewhat from that of Marsland. Ten grams of Methocel, with a viscosity rating of 15 centipoises, are mixed with 90 ml. of distilled water. The viscosity of Methocel solutions in centipoises is based on the viscosity of a 2.0 per cent aqueous solution at 20° C. The centipoise values of the six viscosity types are: 15, 25, 100, 400, 1,500, and 4,000. The Methocel is dissolved most easily by first mixing it thoroughly with half the required water at boiling temperature and allowing it to soak for 20–30 minutes. The remaining water may be added as cold water. The mixture is then cooled to room temperature and stirred until smooth. Methocel solutions of maximum clarity are obtained by reducing the temperature to 10° C. after thoroughly wetting the Methocel fibers with hot water. Methocel is unique among the hydrophilic colloids in that solutions of it gel on heating.

Solutions may also be prepared by stirring Methocel in water at room temperature. If bubbles are observed, the mixture should stand until these disappear. The advantage of an initial hot-water treatment is that it hastens dispersion, and cooling to 10° C. insures a haze-free, colorless solution. Aqueous solutions of Methocel are stable on long standing and usually do not require a preservative.

After much experimentation with Methocel, we find that the most satisfactory method of making preparations for laboratory studies is as follows: Make a ring of Methocel about $\frac{1}{8}$ inch wide, with a diameter a little smaller than a $\frac{7}{8}$-inch cover glass; place a small drop of paramecium culture in the center of the ring; then add cover glass. This gives the observer an opportunity to see the unhindered movements of paramecia before they are slowed down by diffusion of methyl cellulose. As the Methocel diffuses, there is a progressive increase in viscosity of the culture medium which gradually slows the paramecia until they are practically immobilized. Brown (*1*) used 5 per cent Methocel for the ring on his slides, but he does not give the viscosity type used.

Under the conditions provided by a Methocel preparation, a paramecium can be observed leisurely and critically. The activities appear to be normal but much slower, and such preparations have the fascination of a slow movie for beginning students. The Methocel technique also makes possible an excellent demonstration of Brownian movement of protoplasmic granules in a paramecium.

Instead of using carmine to observe the passage of food vacuoles throughout the body, as Marsland (*3*) suggests, we use a mixture of yeast and congo red, recommended by Buck (*2*) for immobilizing paramecia. While we discontinued Buck's method of quieting paramecia, because it did not always suffi-

[2] Methocel used in our experiments was generously supplied by the Dow Chemical Company, Midland, Michigan.

ciently immobilize, we found it excellent for observing physiological processes.

Various stages of feeding can be observed, including the ciliary beat and currents into the gullet, and the formation of the food vacuole, its elongation and pinching off, and its course through the cytoplasm. The principle of cyclosis is well illustrated by the movement of the beautifully colored food vacuoles in the cytoplasm. Inasmuch as congo red is a chemical indicator, it gives information regarding the chemical changes occurring in the food vacuoles. Usually the food vacuole is bright red when it is first formed, then changing to blue, which indicates an acid reaction (pH 3.0 or below), then to purple in the intermediate stage, and finally to an orange-red, which appears at a pH of 5.2 or above. Actually, the food vacuole is known to be alkaline in the later stages of digestion.

Even the beginning biology student with little interest in biology and inexperienced in microscopy finds it exciting to observe the pulsating vacuole, gullet, undulating membrane, cyclosis, changes in the color of the food vacuoles, defecation, trichocysts, and other features of physiology and structure. In fact, it has been reported that these studies of paramecia create so much interest and enthusiasm for beginning students that they are a common topic for dormitory discussion.

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The Purification of Diphtherial Toxoid[1]

Louis Pillemer and David Toll

Institute of Pathology, Western Reserve University

Highly purified diphtherial toxoid has been separated in this laboratory by methods similar to those employed for the crystallization of tetanal toxin (*7*) and for the purification of tetanal toxoid (*4*, *5*). The purified diphtherial toxoid contains between 2,000 and 2,200 Lf/mg. of protein nitrogen. This value agrees with that reported by Pappenheimer (*3*) and by Eaton (*2*) for purified diphtherial toxin. The only diphtherial toxoid of comparable purity which contained 2,000 Lf/mg. of nitrogen is that reported by Boivin (*1*), who employed precipitation with trichloracetic acid. However, Pope and Lingood (*8*) were unable to confirm Boivin's results.

The parent toxoid employed for purification was prepared by detoxifying potent diphtherial toxin produced on a deferrated semisynthetic medium by a special, highly toxigenic strain of *Corynebacterium diphtheriae* (supplied by Lederle Laboratories). The methods employed for the purification are summarized briefly in Tables 1 and 2.

By balancing precisely the charged condition of the toxoid and the solvent effect of the electrolytes with the precipitating action of methanol at low temperatures, diphtherial toxoid separates from nearly all of the culture medium constituents and bacterial products. In fact, as shown in Tables 1 and 2, a single precipitation at pH 5, ionic strength of 0.09 in 40 per cent methanol at −5° C., yields a product containing 900 Lf/mg. of nitrogen. In Method I, the porphyrins which are present in the parent toxoid were removed from Fraction 3P by adsorption on asbestos fibers at pH 6.6. Fraction 4P was dried from the frozen state and dissolved to a syrup in 0.04 M sodium acetate, the pH adjusted to 5.6 with acetic acid, and the precipitate discarded. The supernatant was adjusted to pH 5.3 at 0° C. and the precipitate again discarded. Fraction 6S contains between 2,000 and 2,200 Lf/mg. of nitrogen and exhibits a much shorter Kf than that of the parent toxoid when tested at identical Lf concentration. On standing at 0° C., poorly defined protein crystals formed in Fraction 6S. The precise conditions necessary for crystallization are being investigated further. An alternative purification procedure, given in Table 2, eliminates both the adsorption of the porphyrins on asbestos fibers and multiple dryings from the frozen state. In this method, the porphyrins and bacterial proteins are largely removed as a precipitate at pH 4, ionic strength of 0.075 in 5 per cent methanol at −2° C. The toxoid principle is then precipitated as indicated under 4P and 5P. Whereas the remaining bacterial proteins are soluble under these conditions, the toxoid principle precipitates, resulting in a final product containing over 2,100 Lf/mg. of nitrogen. The decreased flocculation time of the purified toxoid as compared with the parent toxoid indicates that no damage to the toxoid principle occurs under the above-stated conditions.

TABLE 1

Preparation	Conditions of separation				Lf/mg. N	Kf* (min.)	Yield (% of parent toxoid)
	pH	Methanol (%)	Ionic strength	Temperature (C.)			
Parent toxoid	—	—	—	—	10	45	—
1 P†	5.0	40	0.09	−5	900	30	100
2 P	5.35	25	0.11	−5	1,280	25	95
3 P	5.85	40	0.11	−7	1,440	25	95
4 P	5.4	40	0.06	−5	1,600	20	85
5 S‡	5.6	—	0.04	0	1,740	20	75
6 S	5.3	—	0.03	0	2,090	20	70

* Tested at 35 Lf units.
† P indicates precipitate.
‡ S indicates supernatant.

TABLE 2

Preparation	Conditions of separation				Lf/mg. N	Kf* (min.)	Yield (% of parent toxoid)
	pH	Methanol (%)	Ionic strength	Temperature (C.)			
Parent toxoid	—	—	—	—	10	55	—
1 P	5.0	40	0.09	−5	880	40	100
2 P	5.8	40	0.09	−7	1,260	40	95
3 S	4.0	5	0.075	−2	1,840	20	70
4 P	4.1	30	0.05	−6	1,980	20	65
5 P	5.35	25	0.075	−5	2,120	20	65

* Tested at 50 Lf units.

The employment of stabilizing agents and of special precautions in filtration and in drying from the frozen state has

[1] Aided by a grant from the Lederle Laboratories.

led to the quantitative recovery of a highly pure, stable, antigenic toxoid which should prove suitable for clinical use (*6*).

The purified toxoid is not precipitated by an anti-*C. diphtheriae* rabbit serum and is relatively free of porphyrin. It gives the usual protein reactions, and in general its characteristics are almost identical to those of the purified toxin prepared by Pappenheimer (*3*). The details of the purification procedures, as well as the characteristics of the purified toxoid, will be reported later.

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A Simple Anaerobic Method of Obtaining Plasma

Horace W. Davenport

Department of Physiology, University of Utah School of Medicine, Salt Lake City

By the method described here one can obtain plasma for gas analysis without the inconvenience and uncertainty of transferring the blood from the syringe into which it is drawn and

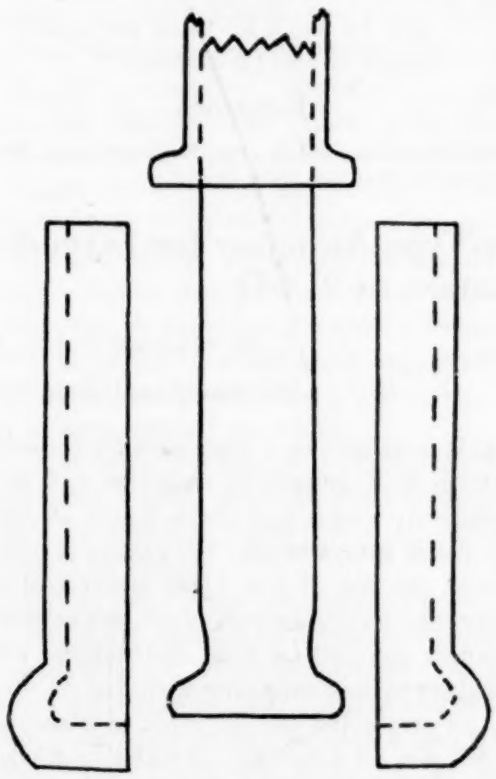

Fig. 1

without the use of oil or mercury. The method is described for a 5-ml. syringe, but syringes of any size can be used.

About 5.5 ml. of blood is drawn into a 5-ml. syringe containing some heparin solution and greased with stopcock lubricant. The needle is removed, and about 1 ml. of blood is delivered for estimation of the hematocrit, pH, etc. A short piece of thick-walled rubber tubing is slipped over the nozzle of the syringe. The two halves of the plastic spacer shown in Fig. 1 are placed around the plunger and held with rubber bands, the plunger then being pushed in until stopped by the spacer. The length of the spacer is such that the syringe contains about 4 ml. of blood. The lumen of the rubber tubing is now full of blood; and while the spacer is held firmly against the barrel of the syringe a glass plug is inserted into the lumen, displacing the blood. The syringe is centrifuged, plunger down, at about 1,500 r.p.m. in a 50-ml. cup fitted with a reducing ring.

When centrifugation is finished, the spacer and the glass plug are removed. The tip of a blood-gas pipette is inserted into the lumen of the tubing, and, by means of gentle twisting pressure on the plunger, plasma is delivered directly into the pipette.

"Braking" Pipettes

C. Lloyd Claff

Laboratory for Surgical Research, Harvard Medical School, and Marine Biological Laboratory, Woods Hole

A "braking" pipette is an extremely important tool in many microtechniques. A good braking pipette, because of its slow, controlled rate of flow of air, enables the investigator to pick up several single cells in a measured amount of fluid as small as 0.5 mm.[3]

The conventional type of braking pipette, constructed by pulling a fine, hair-like constriction in a capillary pipette, is very unsatisfactory. If any moisture collects in the constriction, it is almost impossible to clear it. Even if it is baked out, the residue left in the constriction usually changes the characteristics of the pipette, *i.e.* the rate of flow. It is unusual to make two of these with the same characteristics, since a good one is usually the result of a happy accident. An improvement over the constriction pipette has been described by Linderstrom-Lang and Holter (*1*), but their construction does not solve the problem of condensation in the brake, because they use a continuous capillary system such as that used in previous constriction pipettes.

The following two types of braking pipettes have proved easy to make; characteristics of one type can be duplicated in any number of pipettes, and both types are satisfactory in operation.[1]

Type A: The chief advantage of this type, in which a replaceable glass capillary brake is used, is that the brake can be replaced when necessary. For instance, if the pipette is calibrated, and a change in rate of flow is required, the brake can be replaced without recalibrating the tip. Furthermore, the possibility of moisture collecting in the brake is eliminated, in as much as the latter is not part of a continuous capillary system.

A piece of heavy pyrex capillary tubing, 6 cm. long and with an internal diameter of 0.5 mm., is joined to a capillary tube of the same length and external diameter but with an internal bore of 1.0-mm. diameter. The end of the 1.0-mm. capillary tube is flared as shown in Fig. 1a. A 0.3- to 0.5-mm. capillary

[1] I wish to acknowledge my indebtedness to Mr. James Graham, University of Pennsylvania Medical School, Philadelphia, for his excellent suggestions and cooperation in connection with the development of these pipettes.

tube is pulled to a finely drawn-out braking tip. A globule of low-melting-point, transparent, sealing resin[2] is melted onto the capillary near the end opposite the brake. The capillary tube is then dropped into the lumen of the pipette and heated cautiously over a microflame until the resin makes a seal at point

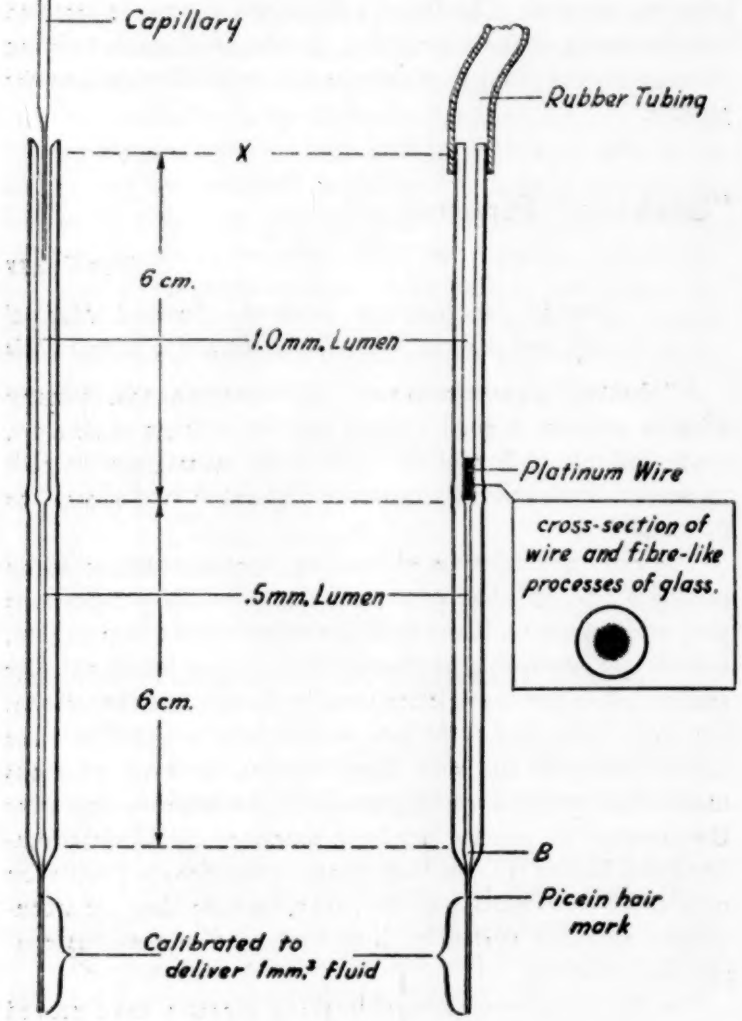

FIG. 1a FIG. 1b

X. The pipette is completed by pulling a tip in the conventional manner. A convenient way to mark the calibration on the tip is to pull a fine thread of heated black picein to a fine hair, winding it in a circle around the capillary tube at the calibration point before it is entirely cool. Then, by bringing the point where it is applied near a microflame, it will be sealed to the glass. The capillary brake may be cut close to the resin seal, and the pipette is ready for attaching the rubber tubing to complete a mouth-suction pipette.

Type B: The rate of air flow is controlled by taking advantage of the slight difference of the coefficient of expansion between platinum wire and pyrex glass. The two main advantages of this type are that (1) any number of pipettes with the same characteristics may be made by using the same length and diameter of platinum wire in the brake, and pyrex capillary tubing from the same lot of glass; and (2) fluid as well as moisture, if drawn into the brake, may be baked out over a microflame, and the pipette dried in a hot-air oven without materially changing the characteristics of the pipette.

A piece of heavy-wall pyrex capillary tubing 6 cm. long, with an internal diameter of 0.5 mm., is fused to a heavy pyrex capillary tubing of the same length, with an internal diameter of 1.0 mm. A piece of platinum wire 8 mm. long, with a diameter of .017 mm. (# 25 gage), is inserted at point X (Fig. 1b) and worked down to the point where the two inside-diameter bores meet. The pipette is then heated at that point in an oxygen-air-gas flame until the platinum wire is white hot. The capillary tubing will shrink around the white-hot wire. Care must be taken not to fuse the capillary bore closed at either end of the wire. This can be avoided by adjusting the blowtorch to a rather sharp flame. The pipette should be held in this flame for about a minute after the capillary has shrunk onto the platinum wire, the capillary allowed to cool to a cherry red in an air and gas flame for about another minute, and then the capillary "smoked" in a gas flame and allowed to cool to room temperature. As the pipette cools, the difference in coefficient of expansion of the platinum wire and pyrex glass causes the platinum wire to shrink away from the glass. The space between the wire and the glass will become filled with fiber-like processes of glass, which becomes the air "brake."

The tip is pulled in the ordinary manner, except that bulb B (Fig. 1b) has to be blown from the tip end of the pipette instead of the open end marked X. (The tip may be formed, if desired, before the platinum wire is inserted.)

A pipette made in this manner has a smooth, slow movement of fluid in the tip, even when a maximum of suction is applied by sucking on rubber tubing applied to point X. A slightly faster pipette may be made by using platinum wire 8 mm. long, with a diameter of .026 (# 22 gage).

[2] Transparent sealing resin (see *Chemistry and physics hand book* (23rd ed.), p. 2002.

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A New-Type Atomizer for Large-Scale Application of 2,4-D

C. L. HAMNER and H. B. TUKEY

Michigan State College, East Lansing

If 2,4-D is to be used on a large scale for the eradication of weeds on range land, grainfields, roadsides, and similar situations, a relatively simple and cheap means of application is necessary. Water sprays require 120 gallons of water per acre when used at the rate of 1 to 1,000. Because of the lack of ample water supplies and because of the weight of the material and equipment required for their distribution, water sprays offer difficulties in any extensive operation of weed control. However, by employing the principle of atomization using concentrated oils, oil emulsions, or water solutions of 2,4-D, application can be made at as low a rate as 3 gallons per acre or less. Atomization of the solution is accomplished by passing air at low pressure (2 pounds) over the solution to be atomized. It is broken up into finely divided particles which form a mist.

The most important feature of the apparatus is the atomizing nozzle, a pattern for which was furnished by the Shell Oil Company of New York City through its representative, C. S. Harris. The nozzle is of brass, 1 $\frac{11}{16}$ inches long, $\frac{7}{8}$ inch wide, and

1¾ inches deep, with separate openings on one edge for an air stream and for the spray material. A chamber inside the nozzle is so constructed that the stream of air flows over a well of material and is blown out from the opposite side in a flat, fan-shaped spray in an arc of about 40°. The openings for the ejection of the spray are on the edge of the nozzle opposite to the openings for entry.

The nozzles are connected on a spray boom 12 inches apart in 6-foot sections. The support for the boom is furnished by the air line, which is a steel pipe ½ inch in diameter, and by an oil line ⅜ inch in diameter. The oil line is under slight pressure (1–5 pounds), and the air line delivers 2 pounds. The delivery rate of the nozzle is easily adjusted by controlling the feed of the solution with a valve. Adjustments can be made to distribute as little as 1 pint over an acre.

A ¼-horsepower gasoline engine, to operate a small air compressor, can be placed on a ½-ton truck.

The apparatus here described makes feasible the return of range land to a grass dominance, which, due to overgrazing, has been shifted to other plants. Also, on roadsides, along fence rows, and on right-of-way of such utilities as power companies and railways, the dominance may be changed to grass by the elimination of woody plants, climbers, and tall, undesired vegetation.

Detection of Serum Agglutinins for *Monilia* and Other Yeast-like Organisms

R. F. Norris and A. J. Rawson

Laboratory of Clinical Medicine, University of Pennsylvania

In the detection of serum antibodies for *Monilia* (*Candida*) *albicans* and other yeast-like organisms, the agglutination test is not widely used in clinical laboratories. When the agglutination reactions of these organisms have been studied in man and experimentally immunized animals, discrepancies in the results have cast doubt on the value of the procedure for diagnosis. Since in most of the reports the method employed has not been given in detail, this difficulty may be due in part to differences in technique. Another objection has been the reported frequency of spontaneous agglutination of the antigen (*3*, *5*, *6*). Consequently, there is need for a standardized, reliable, agglutination procedure.

For some time we have employed an agglutination technique which differs only slightly from the usual methods of determining bacterial agglutinins and which in our hands has proved consistently reliable.

The *Monilia* or other yeast-like organism to be used as an antigen is heavily inoculated on Sabouraud's glucose agar, which is incubated at room temperature (approximately 20°C.) for not more than 24 hours. At the end of this period there is usually sufficient growth to make a heavy suspension in physiological salt solution. The organisms, suspended as an antigen, are then killed by heat at 60°C. for 1 hour.

The concentration by volume of the original suspension is determined by centrifugation in a graduated tube, and the suspension is then diluted with physiological salt solution to the optimal concentration.

Serial dilutions of the test serum, previously inactivated at 56°C. for 30 minutes, are made up, and equal parts of the antigen suspension are added to each tube. For each group of tests, known negative and positive sera are set up as controls in the lowest dilution to be employed in the tests. In addition, we customarily use a saline control.

The tubes are incubated at 56°C. for 2 hours, after which they are placed in the refrigerator overnight and then read. For this purpose individual tubes are held in a nearly horizontal position over the concave surface of a microscope substage mirror placed on a table top before a window or a strong light. The tube is tapped gently with the finger to agitate the contained particles. The images of the particles reflected in the mirror are slightly magnified and can be readily observed.

In a negative test there is a uniform suspension of small, translucent, gray particles which are not agglutinations since they have one or more buds.

In a positive test complete agglutination is shown by a dense, opaque, white, flocculent precipitate which is broken up into large, irregular clumps on agitation. This reaction is considered to be 4+; 3+ is indicated when a flocculent precipitate breaks up into smaller and more uniform coarse white particles; a sediment of easily suspended, large white granules is called 2+; a suspension slightly larger and more opaque than the negative controls is called 1+.

The optimal concentration of the antigen is determined by titrating serial dilutions of an antigen suspension against serial dilutions of an immune serum. Immune serum is prepared by injecting rabbits intravenously with heat-killed, 24-hour saline suspensions of organisms by the method of Benham (*1*).

The use of a 24-hour culture of the yeast-like organism, as employed by Kesten, *et al.* (*4*), is highly important to avoid the presence of mycelia, which in many instances appear to be the cause of spontaneous agglutination.

By employing optimal concentrations of antigen, agglutinin titers are more apt to be constant and, presumably, maximal.

The use of the substage microscope mirror for reading the tests eliminates the uncertainty of end-points and requires much less time than do microscopic readings.

In repeated microscopic checks of our macroscopic readings, we have found that, in 1 +, there are clumps of 10–100 organisms; in 2 +, clumps average about one-third the diameter of an oil immersion field; and in 3 and 4 +, there are gross clumps.

In routine tests of human and rabbit sera there has been no evidence of spontaneous agglutination, and zone reactions have not occurred when previously inactivated sera were used (*2*).

The titers of antibodies in the same individual, when repeated at short intervals, are constant, and the majority of different strains of a species are agglutinated in the same titer by an individual serum. Among different strains of any one species, variations greater than one tube in titer have not been observed.

The method has been found to be reliable in the study of several species of *Monilia* and *Saccharomyces* and in the study of *Willia anomala*. It has not been found reliable in the study of organisms from which mycelial filaments cannot be eliminated.

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Book Reviews

Allergy. (2nd ed.) Erich Urbach and Philip M. Gottlieb. New York: Grune & Stratton, 1946. Pp. xix + 968. (Illustrated.) $12.00.

In this second edition Dr. Gottlieb, who assisted in the production of the first edition, becomes co-author.

Despite an increase in reading matter, this edition is less bulky than the first, due to adoption of a double-column format and use of small type for certain sections.

The number of illustrations of allergic skin manifestations continues to be excessive. One questions the need of 9 illustrations to portray angioneurotic edema or of 14 to demonstrate urticaria, not to mention manifold picturizations of fixed drug eruptions, lichen urticatus, and contact dermatitis. Fig. 29 is supposed to illustrate urticaria but is not obvious as such; while Fig. 296, showing necrosis and scarring of the skin due to injections of adrenalin, duplicates Fig. 157.

As with the first edition, the outstanding feature of the work is its comprehensiveness (although a surprising omission in both editions is that of a section on preparation of extracts). A more detailed review of the literature, and particularly of the foreign literature, is not to be found in any other similar text. Unfortunately, the authors do not always evaluate the literature critically. For example, in the section on the symptomatic therapy of allergic rhinitis one finds the casual statement that the following methods have been recommended for general therapy: injections of histamine, peptone, tuberculin; vaccines of colon bacilli, bee venom, snake venom; and oral histaminase. This statement is followed by the presentation of Beckman's nitrohydrochloric acid prescription in detail, which in turn is followed by the statement that a trial of the ketogenic diet has been recommended—all this without critical comment. Mention of these methods no doubt adds to the comprehensiveness of the book and may be permissible as a matter of historical interest, but the inexperienced reader is entitled to know that these nonspecific methods of therapy have long been discarded, if indeed ever used, by allergists of repute.

Furthermore, present-day practice would frown on the use in a single prescription of such a polypharmaceutic mixture as the following, which is recommended by the authors (p. 643): codeine, ephedrine, belladonna, lobelia, potassium iodide, elixir terpin hydrate, or (p. 651): ephedrine, caffeine, digitalis, belladonna, phenobarbital, aminophyllin.

Also amiss is the continued promulgation by the senior author of certain matters which have so far remained largely unaccepted by other allergists. First may be mentioned his terminology, which produces such coinings as "allergize," and such tortured phrasings as "hetero-allergic pathergy," "polyvalent metaspecific allergy," and "heterospecific deallergization." Second is his technique of treating food sensitivities by means of so-called "propeptans," which are digests derived from foods by treatment with hydrochloric acid, pepsin, and trypsin. Third is his advocacy of oral pollen and pollen propeptan therapy.

For the foregoing reasons, one would hesitate to place this book in the hands of a beginner who needs orientation in the field of allergy.

These criticisms aside, the work reveals much that is of genuine merit. The advanced student will find that the detailed presentation of the factual material in many sections in Part 2, on the etiologic agents of allergic diseases, and in Part 3, on symptomatology and therapy, cannot be excelled elsewhere. And to the experienced allergist it remains, withal, a valuable compilation of the literature.

A. M. Targow

Department of Medicine, Division of Allergy,
University of Southern California School of Medicine

L'origine des cellules reproductrices et le problème de la lignée germinale and ***Continuité germinale et reproduction agame.*** L. Bounoure. Paris, France: Gauthiers-Villars, 1939; 1940. Pp. xii + 271; pp. 83. (Illustrated.)

Bounoure's companion volumes, distribution of which was arrested by the war, have now been made available to libraries through the courtesy of the Services du Conseiller Culturel of the French embassy.

These volumes are particularly interesting because they bring into sharp focus two generalizations in biology which are of the highest importance and which should, and probably will, change the fundamental outlook in most of the biological sciences, particularly the applied sciences—the morphological continuity of the totipotent germ cells, and the law of the restriction of cellular potency or competence.

Bounoure has crystallized the conclusions gradually taking shape in the minds of biologists and has demonstrated that a morphological continuity and precocious segregation of the germ cells can no longer be doubted, thus ending a controversy which began after Richard Owen first enunciated the idea in 1843. Belated and much deserved credit is given to John Beard for having championed in his usual logical and forceful manner the morphological continuity and precocious segregation of germ cells at a time when the great majority of biologists were firmly aligned on the side of Weismann and Waldeyer, who reversed his position later, however. Bounoure quotes the following from Beard: "To us as embryologists and men the formation of an embryo has appeared to be everything, the history of the germ-cells a secondary item of no particular moment. Nature, on the other hand, reverses the relative importance of the two, setting the germ-cells on the place of honor, as linking the remote past with the distant future." There is much more for which Beard deserves belated recognition. The tribute which is paid him by Bounoure is a hopeful sign that science, which sometimes buries the work of a genius, does not bury it forever.

The manner in which Bounoure develops his subject makes exciting reading. He examines a wealth of evidence, beginning with the Protozoa and continuing through Volvox, Ascaris, the insects, Crustacea, Chaetognatha, Rotifera, Annelida,

mollusks, and the vertebrates. A remaining resistance against accepting morphological continuity in mammals is no longer necessary since N. B. Everett's work on the mouse (*J. exp. Zool.*, 1943, **92**, 49–91).

Regarding the supposed origin of sex cells (gonocytes) from the germinal epithelium, Bounoure writes the following: "To this question many workers have answered in the affirmative; but their arguments are derived from *pure observations* (italics, C. G.) and usually consist of the supposed existence of transition stages of coelomic cells into germinal cells." These transition stages are really slices inevitably obtained by sectioning the large germ cells.

The second volume discusses asexual reproduction as it applies to germinal continuity. After evaluating a great many data concerning those forms in which asexual reproduction is best illustrated, the author shows that the germ cells are involved in both sexual and asexual reproduction.

The discussion of the law of the restriction of cell potencies, begun in the first volume, is forcefully and convincingly developed. Bounoure states: "One of the most important results of tissue culture is to *have demonstrated the irreversible character of cellular differentiation*. The consequence of these results destroys every hypothesis which attributes to cells *in physiological isolation from the organism*, the possibility of reverting to the embryonic state and to acquire through this reversion new and greater potentialities." These conclusions are of great importance to the cancer problem.

There are 555 references in the first volume and 171 in the second, but no indexes.

CHARLES GURCHOT

The John Beard Memorial Foundation,
942 Capp Street, San Francisco

The chemistry of heterocyclic compounds. Avery A. Morton. New York: McGraw-Hill, 1946. Pp. vii + 549. (Illustrated.) $6.00.

The literature of heterocyclic chemistry, barring the publications of original investigators in the field, is confined to a relatively small number of specialized monographs and a variety of survey chapters in some of the larger works on organic chemistry. This book is a contribution which has not been attempted previously; it is a textbook of heterocyclic chemistry and will prove useful in presenting graduate courses in that subject. The book is not intended as a reference work, but the comprehensive manner in which it reviews the third broad division of organic chemistry, and the representative quality of the numerous literature citations, make it a work of interest to organic chemists generally.

The material is arranged in classical order, oxygen and sulfur heterocycles preceding the more numerous nitrogen compounds, simpler ring systems being followed by structures of increasing complexity. Most of the chapters are prefaced with historical remarks of human interest, after which the nomenclature, preparation, and reactions of compounds containing the ring system under discussion are introduced. Descriptions of representative compounds and a selection of problems follow. Emphasis is placed on natural products, and the compounds selected for description either occur naturally or are of theoretical significance. The problems are a welcome addition to a text of this type and alone cover an appreciable part of the literature. Many of them reconstruct syntheses which have been realized experimentally, while others illustrate degradative methods of structure determination. Statements of problems include a starting material, a sequence of reagents, the composition of the final product, and a reference to the paper in which the reactions are described.

The typography of the book is identical with that of preceding volumes in the International Chemical Series. Misprints are few and self-evident, although errors like parabamic for parabanic acid (p. 462) could prove misleading. The use of a large R for a condensed benzene ring, "to focus attention on the heterocyclic chemistry of a synthesis," is initially confusing and without increased significance. Analytical applications are mentioned for many of the compounds described, and the omission of a substance like picrolonic acid is surprising. Not all heterocyclic compounds are difficult to prepare, and the ready availability of some of them compared with similarly constituted but theoretically simpler carbocyclic and aliphatic substances is of general interest. The accessibility of many unusual aliphatics and aromatics through syntheses involving heterocyclic intermediates is just touched (pp. 130 and 431).

FRED E. SHEIBLEY

University of Kentucky, Lexington

The prolongation of life. Alexander Bogomolets. (Translated by Peter V. Karpovich and Sonia Bleeker.) New York: Duell, Sloan & Pearce, 1946. Pp. xvii + 98. $1.50.

The Russian edition of Bogomolets' *The prolongation of life* has been read by several million people, and brief popularizations of phases of his work have appeared in the *Ladies' Home Journal* and in *Reader's Digest*. In making available this translation to the non-Russian reader, its purveyors astutely preface the work with the statement that they act "merely as transmitters." The author, the late Alexander Alexandrovitch Bogomolets, was president of the Ukrainian Academy of Sciences, founder and director of the Kiev Institute of Experimental Biology and Pathology, and recipient of a Stalin Prize, first class (March 13, 1941).

Since this book is primarily a popular essay on the subject of aging, no attempt is made to present a critical discussion of antireticular cytotoxic serum (ACS).

The bibliography of the text is very limited, and none of the work on gerontology in the English language is quoted. Nine different allusions, however, are made to Hufeland's 1796 edition of *Makrobiotik, oder die Kunst, das manschlice Leben zu verlängern* (translated into Russian in 1852). Most of the data on specific cases of longevity are quoted from the pages of *Izvestia*, *Pravda*, or the newspaper *Communist*. There are other miscellany on examples of longevity in plants and animals. While these data cannot be accepted as scientific material, they are of such a nature as to endow the manual with some value as a source book for *curiosae* on longevity.

Regardless of the validity of Bogomolets' hypothesis, his popular manual can serve only to perpetuate among the laity an alarming quantity of pseudo science and old wives' tales. On page 26 he writes: "I stated a long time ago that cancer cannot develop in an organism if the reticulo-endothelial tissue has retained a sufficient degree of resistance." Since the occurrence of cancer is then accepted *ipso facto* as an indication of the failure in resistance of the reticulo-endothelial system, Bogomolets' statement seems to mean no more than that, for

people who like this sort of thing, this is the sort of thing those people like. On page 31 "fatal autointoxication" is mentioned, though western medicine has generally abandoned the unproved hypothesis of "autointoxication." On page 35 we read: "It is really possible to 'die of happiness, or of grief.' " Hufeland is then quoted authoritatively (p. 36) to the effect that " 'there are instances of people who, subjected to fear, turned completely gray in the course of one night.' " On page 42 the exploded myth of "ptomaine poisoning" is added to the folklore of "autointoxication." On page 44 Hufeland is again quoted: " 'The crow lives to be one hundred . . . the rooster to a decidedly shorter age, because he is such a tempestuous, quarrelsome, and lustful fowl.' "

On the positive side we might mention Bogomolets' drawing attention to the somewhat neglected work of Élie Metchnikoff; this, however, cannot abrogate the seriousness of circulating among uncritical readers a book so replete with error as this. Certainly, hypothetication of the boldest type is not to be criticized, but even dialectical materialism must not be expected to afford amnesty to error in simple scientific facts.

ERNST T. KREBS, JR.

University of California Medical School, San Francisco

Scientific Book Register

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